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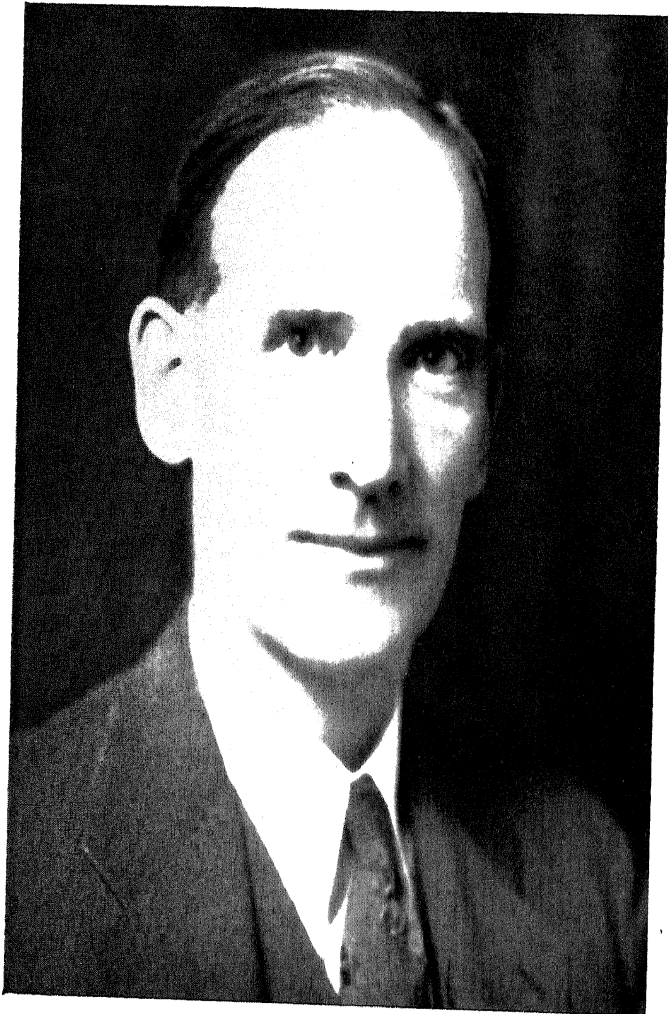
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J. G. Cottrell.

FREDERICK GARDNER COTTRELL

1877-1948

BY VANNEVAR BUSH

The words in which the Western Society of Engineers in 1937 cited Frederick Gardner Cottrell for the Washington Award are a tribute and an estimate that well merit repetition. That Award was presented to Dr. Cottrell "for his social vision in dedicating to the perpetuation of research the rewards of his achievements in science and engineering." Here are summed up the past accomplishments and the continuing benefactions of a long and distinguished career which was marked throughout not only by vigorous pioneering initiative in physical chemistry and industrial engineering but also by a constant and lively sense of the social significance of scientific and engineering advances and the social responsibility of the scientist and engineer. Coupled with this sense of social responsibility was recognition—conviction, indeed—of the intrinsic value and the human utility of research.

Out of this combination there came in 1912 the creation of a unique enterprise, characterized in the beginning by its initiator as an experiment in the public administration of patent rights. It was an innovation, a fire-new pioneering idea, and out of it has grown over the years a new philosophy concerning the utilization of the results of creative endeavor. We know it today as the Research Corporation, a corporate entity which is almost a contradiction in terms, since it is at once a business and a non-profit undertaking. The Research Corporation was founded through gifts by Dr. Cottrell and his associates of valuable rights in patents in the field of electrical precipitation, for the purpose of utilizing the proceeds from applied research for the further advancement of science and technology. The Corporation, operating in the same field and in the same general fashion as any private business, pays no dividends to personal stockholders; rather, all its income above expenses—apart from what is needed for reserves or for operating capital—is ex-

pended to assist scientific and educational institutions in carrying on research.

The social philosophy underlying the Research Corporation was never better stated than by Dr. Cottrell himself in 1937, in his address "The Social Responsibility of the Engineer," delivered in acceptance of the Washington Award. Referring to the Research Corporation and its offshoot, Research Associates, Inc., which was active from 1935 to 1938, Dr. Cottrell said: -

"Briefly these two corporations may be said to hold a place and typify a new class midway between such privately endowed research and welfare foundations as Carnegie, Rockefeller, Russell Sage, Rosenwald, Macy, and the like on the one side, and university research foundations, such as those at Wisconsin, Purdue, and more recently Ohio State on the other. They differ from the first category in starting with no large monetary endowment, that interest or dividends on securities purchased as investment are not intended ever to be a significant factor in their income, but that normally they earn their way currently through services in invention development and production for use. They differ from the university foundations on the other hand chiefly by being entirely free from commitments to any one institution and thus being able to work with any and all of them, either individually or co-operatively. Also, I think it is still fair to say, though this I do with much reservation, that as yet most of the university foundations have been conceived and administered more largely from the standpoint and hope of revenue for further scientific research in the universities than from that of a laboratory in social economics, which I have particularly tried to emphasize as the outstanding opportunity among the purposes of Research Corporation and the Associates. That is, they are frankly willing to risk or even sacrifice on occasion possible legitimate profits from the licensing or operation of patents or developments if thereby a more important public service can be rendered by demonstrating the relative value and pertinency of proposed reforms in business and social administration of such rights, monopolies, or other social-economic structures as the corporation may control or operate at a given time."

The work which gave the Research Corporation its original financial support, and which in 1919 brought to Dr. Cottrell the Perkin Medal of the Society of Chemical Industry, began

in 1906, when he investigated the problem of sulfur trioxide fumes. The successful development of methods for the electrical precipitation of particles of liquids and solids, originating with this study, culminated in the Cottrell precipitator, invaluable in smelters, cement kilns, and wherever fumes and dusts are a hazard to life and an economic waste. This work is covered in patents on processes and apparatus for separating and collecting particles of one liquid suspended in another liquid, or separating and collecting suspended particles from gaseous bodies for the purification of gases or for the abatement of smoke, fumes, and dust.

Initially, rights in six patents were offered by Dr. Cottrell and his associates to the Smithsonian Institution in order that profit resulting from them might be applied to the advancement of scientific research and investigation. The business administration required was outside the range of the Smithsonian's activities, however, and so the Research Corporation came into being. In the years since 1912, it has made millions of dollars available for the support of research, through grants to institutions and individuals, and it has contributed invaluablely through intangible aids such as advice and counsel in the handling of inventions and the administration of patents. Dr. Cottrell himself served in this work not only through the Research Corporation; he was closely associated with the Wisconsin Alumni Research Foundation and was a director of the Purdue University Research Foundation.

In addition to the Washington Award and the Perkin Medal, many other expressions of recognition of his achievements came to Dr. Cottrell. These included the Willard Gibbs Medal of the Chicago Section of the American Chemical Society, the Gold Medal of the Mining and Metallurgical Society of America, the Holly Medal of the American Society of Mechanical Engineers, and the Medal of the American Institute of Chemists. The University of California conferred upon him in 1927 its honorary degree of LL.D.

Dr. Cottrell was a member of the National Academy of Sciences, the American Institute of Mining Engineers, the American Association for the Advancement of Science, and

the American Electro-chemical Society, and an honorary member of the Société de Chimie Industrielle. Phi Beta Kappa, Sigma Xi, and Alpha Xi Sigma also claimed him as a member. His chairmanship of the Committee on International Auxiliary Language of the International Research Council expressed one phase of his wide-ranging interests—the development and propagation of means for simple universal communication.

Frederick Gardner Cottrell was born at Oakland, California, on January 10, 1877, the son of Henry and Cynthia L. (Durfee) Cottrell. Through his mother, he was of Mayflower descent. His early education was in The Horton School at Oakland, and the Oakland High School. He received the bachelor's degree in chemistry from the University of California in 1896, and spent the following year at the University as Le Conte Fellow. During his undergraduate years, he showed such energy and aptitude that by faculty vote he was given a duplicate key to the laboratories and was authorized to come and go at his pleasure and to carry on any experiments and research he desired.

He made good use of this privilege during the three following years while serving as a teacher of chemistry in the Oakland High School, going then to Germany for further study. In 1900-1901, he studied under van't Hoff at the University of Berlin, and the next year with Ostwald at the University of Leipzig, where he received the doctorate of philosophy in 1902. There followed a return to the University of California where from 1902 to 1906 he was an instructor in physical chemistry and from 1906 to 1911 an assistant professor.

In 1904 he married Jessie M. Fulton of San Francisco. Their two children died in infancy. Of Dr. Cottrell's career as a teacher *The California Alumni Weekly* in 1913 remarked, "He was a good instructor; he possessed the power of interesting his students and many of our best graduates who have made a success of their work, either in universities or in technical life, received their inspiration from him. He made companions of his students, without formality, and they felt a genuine respect."

Dr. Cottrell went from the University of California in 1911 to the United States Bureau of Mines as chief physical chemist

on field duty, serving successively thereafter as chief chemist 1914-15, chief metallurgist 1916-19, assistant director 1919-20, and finally as director commencing in 1920. He resigned from this post on January 1, 1921, to become chairman of the Division of Chemistry and Chemical Technology of the National Research Council, continuing as consulting metallurgist to the Bureau of Mines until 1922. In September of that year, he was called from the National Research Council position to serve as director of the Fixed Nitrogen Research Laboratory of the United States Department of Agriculture.

Till 1927 Dr. Cottrell continued in this post, where he felt the opportunity to direct research without too great a burden of administrative detail was especially valuable. When the Fixed Nitrogen Research Laboratory was reorganized in that year, he remained as chief of the Division of Fertilizer and Fixed Nitrogen investigations of the Bureau of Soils in the Department of Agriculture. When in 1930 he resigned from this post, he continued as consultant to the Bureau, which responsibility he carried for a decade. During the earlier period also he had served as consultant to the Smithsonian Institution in 1928-29.

After the end of the First World War, Dr. Cottrell was chairman of a small committee of scientists and engineers assigned the task of collecting chemical and metallurgical information for the Government. The undertaking involved a tour of nearly all the countries of Europe. During it, Dr. Cottrell not only inquired into the progress of projects for an international auxiliary language for the Smithsonian Institution, but also represented the National Academy of Sciences and the American Chemical Society at the Interallied Chemical Conference in Paris, London, and Brussels. At the latter city he also represented these organizations at sessions of the International Research Council.

Organization of Research Associates, Inc., a non-profit corporation growing out of the activities of the Research Corporation, engaged much of Dr. Cottrell's energies in 1935, and for three years thereafter he served as its president. The purpose of the new enterprise was to conduct scientific and social re-

search and to eliminate as far as possible the time lag between the perfection of scientific ideas and their introduction into the national life. The period of Research Associates' activity, from 1935 through 1938, was a most stimulating one. After the organization was dissolved in 1938, some of its functions were returned to the Research Corporation, and Dr. Cottrell devoted himself to numerous scientific projects mainly by means of liaison work among universities, science groups, and foundations. One of his main purposes between 1938 and the time of his death was the promotion of additional research organizations and the furtherance of closer co-operation between those already in existence. Here his long acquaintance with problems of administration of research and invention, through the Research Corporation itself and the university research foundations with which he had been long associated, was of profound value. The informal and unofficial visits to universities which he made during this period in the effort to keep abreast of research developments in virtually all fields, to become acquainted with the investigators concerned and to give such aid as he could, were stimulating to all.

In 1939, through his advocacy, the University of Wisconsin initiated experiments which might lead to the fixation of nitrogen on an industrial scale. These involved application for the heating and quick chilling of air of the Royster pebble-bed furnace method which was prominent among Dr. Cottrell's studies in earlier years. His publications and patents began in 1906, and include not only applications of the pebble-bed furnace and electrostatic precipitation but also cryogenic separation of gases, laboratory apparatus, the dehydration of petroleum, and the recovery of wastes, among others.

Nitrogen fixation continued among his major interests in the final decade of his life, the process under development by the Food Machinery Corporation at San Jose, California, being a focus thereof. During the second World War, he advised on various projects including the industrial use of domestic manganese resources, the production of magnesium, the manufacture of high-temperature ceramics. The magnesium process at the Kaiser plant in Permanente, California, was of great

interest to him, and in 1942 and 1943, as a consultant for the Reconstruction Finance Corporation, he studied the technological problems it involved. In 1944 he removed from Washington, D. C. to Palo Alto, where he continued to devote time and interest to the fixed nitrogen process in San Jose, the patents on which had been assigned by him to the Wisconsin Alumni Research Foundation.

Dr. Cottrell's death occurred November 16, 1948, during a meeting of the National Academy of Sciences on his well loved campus of the University of California at Berkeley. Appropriately, among the technical papers which he had heard the day before his death was an account of the development of the cyclotron, in which a grant from the Research Corporation had been of early assistance.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

- Am. Chem. J. = American Chemical Journal
 Ann. Rept. Smithsonian Inst. = Annual Report, Smithsonian Institution
 Cal. J. Tech. = California Journal of Technology
 Chem. Met. Eng. = Chemical and Metallurgical Engineering
 Elec. Rev. = Electrical Review
 Eng. Min. J. = Engineering and Mining Journal
 Intern. Cong. Appld. Chem. = International Congress of Applied Chemists
 J. Am. Chem. Soc. = Journal, American Chemical Society
 J. Ind. Eng. Chem. = Journal of Industrial and Engineering Chemistry
 J. Phys. Chem. = Journal of Physical Chemistry
 J. West. Soc. Eng. = Journal, Western Society of Engineers
 Proc. Am. Min. Cong. = Proceedings, American Mining Congress
 Rev. Sci. Instr. = Review of Scientific Instruments
 Sci. Mo. = Scientific Monthly
 Sitzber. kgl. preusz. Akad. = Sitzungsberichte, Koeniglich Preussische Akademie, Akademie der Wissenschaften, Berlin
 Tech. Eng. News = Tech Engineering News
 Trans. Am. Inst. Chem. Eng. = Transactions, American Institute of Chemical Engineers
 Trans. Am. Inst. Mining Eng. = Transactions, American Institute of Mining Engineers
 Trans. Commonwealth Club Cal. = Transactions, Commonwealth Club of California
 Zeit. anorg. Chem. = Zeitschrift für anorganische Chemie
 Zeit. physik. Chem. = Zeitschrift für physikalische Chemie

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Thomas Barbour

THOMAS BARBOUR

1884-1946

BY HENRY B. BIGELOW

Thomas Barbour was born on Martha's Vineyard, August 19, 1884, the son of William and Adelaide (Sprague) Barbour of New York City. In 1906 he married Rosamond Pierce of Brookline, Massachusetts, and his married life was full and harmonious, but saddened by the death of his oldest daughter Martha and of his only son William. During the last two years of his life he was in failing health, following a blood clot that had developed while he was in Miami. He was at the Museum of Comparative Zoölogy as usual on January 4, 1946, and in happy mood at home in Boston that evening. But he was stricken later in the night with cerebral hemorrhage, and died on January 8, without regaining consciousness. He is survived by his wife; three daughters, Mrs. Mary Bigelow Kidder, Mrs. Julia Adelaide Hallowell, and Mrs. Louisa Bowditch Parker; and two brothers, Robert and Frederick K. Barbour.

Barbour prepared for college under private tutors and at Brownings School in New York City. It had been planned for him to go to Princeton, but a boyhood visit to the Museum of Comparative Zoölogy determined him to choose Harvard, which he entered as a freshman in the autumn of 1902. He graduated with the degree of A.B. in 1906, and received his doctorate in 1910.

It has been pointed out in earlier obituaries that the pattern of his life-career as a zoologist, museum director, traveler, sportsman and lover of the American tropics had begun to develop in childhood. His father, a director in the great linen mills of William Barbour and Sons in northern Ireland, often took his family with him abroad on his business trips; and in this way Tom had visited most of the larger natural history museums in Europe by the time he was eight years old. When he was nine years old he caught his first trout in the Adirondack Mountains of northern New York, near Tupper Lake where his father had a comfortable camp on an extensive tract of forest land. Here

he spent many happy summers, with his younger brothers, learning the ways of the woods.

In 1898, when fourteen, he was sent to Florida after an attack of typhoid, to recuperate at the winter home of his paternal grandmother, Sarah Elizabeth Barbour, who was an excellent shot, an expert fisherwoman and a born naturalist. Tom visited southern Florida with her; also the northern Bahamas, the first of his many excursions to the West Indies.

During his college career he not only specialized in zoology but spent nearly all his spare time in the Museum of Comparative Zoölogy, working as a voluntary aid among the collections of reptiles and amphibians under the exacting eye of the late Dr. Samuel Garman. And, as has been recorded elsewhere, he had already resolved that he would someday advance to the Directorship of the Museum then headed by Alexander Agassiz.

He was married in October following his graduation, and immediately thereafter he departed with his bride on an extensive tour, first to Ireland, then to India, to Burma, to the most interesting zoologically of the East Indian islands, including Java, Bali, Lombok, Celebes Borneo, the Moluccas and New Guinea, to China and to Japan. Wherever the honeymoon couple went they worked literally night and day, often with native assistants, gathering specimens of all classes of vertebrates and of insects. Aided in part by family connections in the Far East, but chiefly by their own unflagging energy, they managed to visit many places then difficult to reach. The extent and variety of the collections they sent back to the Museum was truly astounding.

Returning to Cambridge, Barbour settled down to graduate study at Harvard, under the late Professor E. L. Mark, interrupting, however, to visit Chile in 1908 as a delegate to the first Pan American Congress, held in Santiago. His Ph.D. thesis was a monograph on the zoogeography of the East Indies, based in part, of course, on existing knowledge, but largely on the materials he and his wife had themselves garnered in the field. This was published as a Memoir of the Museum of Comparative Zoölogy in 1912, at which time he had 47 scientific papers to his credit. The earliest of them, an annotated list of

reptiles found near a summer camp in New Hampshire, had appeared in 1901, while he was still a schoolboy. Twenty-seven had been on reptiles, the others on birds, mammals, fishes, and more general subjects. The geographic coverage, foreshadowing his life interest in zoogeography, had included East Africa, Burma, Java, Borneo, New Guinea, Indo-China, China, Florida, the West Indies in general, Jamaica, Grenada, Panama, Yucatan, Colombia and Bermuda.

The year 1910 was an eventful one for him; not only did he complete his formal academic studies, but in that year he represented the Association of American Universities at the reopening of the University of Mexico, in Mexico City, and was appointed Associate Curator of Reptiles and Amphibians in the Museum of Comparative Zoölogy. With his usual energy he at once began reorganizing and building up the collections. But, as another of his colleagues has remarked in an earlier biography, his interest in the Museum did not end with his own department—or with his own researches let us add—for the next few years saw him sponsoring collecting trips and traveling extensively himself, chiefly in his beloved American tropics, in the interest of the Museum.

His scientific activities were interrupted during the first world war, while he was engaged in Cuba in special intelligence work for which his fluent use of the Spanish language and his wide circle of acquaintances well fitted him. This work completed, he returned to the Museum with unabated zeal.

In 1925 he was appointed Curator of the Herpetological Department, and in 1927 was chosen Director of the Museum, being third in line from Louis Agassiz, its founder; he was also made Director of the Harvard University Museums as a whole. His early ambition was thus attained. A year later he was appointed Professor of Zoology, and he became Alexander Agassiz Professor in 1944. He was also a member of the council of the University Library and one of the Syndics of the University Press.

Coming to the directorship of the Museum after it had suffered a 17-year period of quiescence and decline in activity following its brilliant achievements under the leadership of Louis Agassiz and his son Alexander, Barbour attacked his new

task with characteristic energy. In the revivification he followed plans long laid, for I think he had never swerved from his belief that he would one day be its head. At the end of a year the building had been modernized, and most of the old galleries roofed across for new work rooms. The entire staff became stimulated by his enthusiasm, the public exhibits which had been sadly in need of care were renovated, rearranged and condensed, thus setting free space that was needed for other purposes. As the years passed the staff was expanded as opportunity offered. A closer liaison was developed between the Museum and the faculty of Arts and Sciences by the appointment of several of the Curators to Professorships.

For the rest of his life Barbour's chief care was for the welfare of the Museum. The immediately tangible result was the vast increase of accessions to the collections for which he was responsible. T. B., as his associates all called him, so injected new life into the institution that it soon regained the high rank as a center of scientific learning that it had held previously, and has since maintained. A decade of activity and of expansion followed, with T. B. always guiding and encouraging his colleagues, with all of whose research projects his extraordinarily wide zoological knowledge gave him an almost uncanny familiarity. But his greatest service to the Museum came through his ability to sympathize with his fellow workers, to encourage them and to forward their researches. Indeed, he had a part in every one of the major contributions that came from the Museum's staff during the years of his directorship. That he was able to do so much for so many is the best tribute to his spirit of comradeship. His fellowman was, after all, the vertebrate species that interested him most. His door was always open to scientific peers, or to beginners who came to the Museum knowing that they would find a friend and counsellor. Many rising biologists in this country and abroad benefited from his encouragement—not a few from his purse as well.

The son of wealthy parents, Barbour contributed large sums yearly to the support of the Museum. We all knew this, but the scale and variety of his contributions to other institutions were not so widely known; nor his assistance to students struggling

through their graduate years, and to colleagues in financial straits,—T.B. made no parade of this.

Notwithstanding his primary devotion to the Museum, he found time and energy to take a very active part in the establishment and maintenance of the Barro Colorado Laboratory in Panama, situated on what was originally a forested hill top. When this area was transformed into an island at the time the region (now Gatun Lake) was flooded because of the construction of the Panama Canal, it became the refuge where a varied and abnormally abundant population of tropical animals gathered to escape the rising waters, and where they have flourished since. Barbour assumed responsibility for a large part of the financial support of the laboratory during its early years, and until it was made part of the Canal Zone Biological Area.

The 400-odd scientific papers, based on observations by many authors at the laboratory during the years when he was taking a guiding part were a source of well-earned satisfaction to him. They might well have been a point of personal pride had he been of a prideful nature.

Barbour also was a prime-mover in the development of the Atkins Institute of the Arnold Arboretum at Soledad near Cienfuegos, Cuba. This is a botanical garden, the gift of Mr. and Mrs. E. F. Atkins of Boston. He served as its Custodian for several years, beginning in 1927 and his visits to Soledad were the high spots of each winter for him.

He served as the President of the Boston Society of Natural History from 1925 to 1927; and of its offspring, the New England Museum of Natural History, from 1940 to 1945. He was the leading spirit in the rejuvenation of the Peabody Museum in Salem; a Trustee of Radcliffe College, of the Carnegie Institution of Washington and of the Woods Hole Oceanographic Institution; a member of the Advisory Board of the Guggenheim Foundation; and a Director of the Fairchild Tropical Garden in Florida, named for Dr. David Fairchild, one of T. B.'s lifelong friends with whom he often stayed while in Florida. He was largely responsible for developments leading to the manufacture of Antivenin for snake bite in North America.

Barbour's favorite subjects of study were the reptiles and

amphibians, on which he was a world-authority. He wrote extensively on the birds of Cuba; on fishes; and his memoir of 1912 on the zoogeography of the East Indies (mentioned above) followed two years later by a companion memoir on the zoogeography of the West Indies, established him as one of the foremost students of the distribution of the land animals of these island groups. He disliked cold weather and as long as his health permitted, made trips each winter to Florida, which he regarded almost as a second home; to Cuba, or to Central America, departing from Cambridge in February as a rule, and returning in May, bringing zoological material to enrich the Museum. There was scarcely a Caribbean island nor any considerable part of the Caribbean mainland that he did not visit at one time or another. "Everything there appealed to him; he liked the people, the climate, the architecture, the scenery, the rich vegetation; the problems of geographic distribution and origin of faunas intrigued him. Spanish was a second tongue to him and his fluent use of that language opened doors that would be closed to the ordinary foreigner."

During the late 1920's and early 30's his winter trips were made with the late Allison Armour on the latter's yacht *Utorana*, to the Bahaman chain, to the islands south of Hispaniola, to Honduras and to the west coast of Mexico. In 1934 he went with his wife down the west coast of Africa and up the east coast, stopping off at many ports and seeing the Kruger Park. They went to Africa again in 1935 to visit the South African game reserves.

He received the honorary degree of Sc.D. from the University of Havana in 1930; from Dartmouth in 1936; from Harvard in 1939, and from the University of Florida in 1944. His memberships, regular or honorary, in learned societies in this country included the National Academy of Sciences; American Academy of Arts and Sciences; American Philosophical Society; American Ornithological Union; American Society of Mammalogists; American Society of Ichthyologists and Herpetologists; New England Zoological Club; Boston Society of Natural History; Nuttall Ornithological Club; Philadelphia Academy of Natural Sciences; Biological Society of Washington; American

Society of Zoologists; American Antiquarian Society; Massachusetts Historical Society; Society of Tropical Medicine; New York Zoological Society; Zoological Society of Philadelphia; and Hispanic Society of America. He was also an honorary or regular member of the Royal Asiatic Society; the Royal Geographical Society, London; the Zoological Society of London; the *Niederlandische Dierkundige Vereen*; the Academy of Sciences of Havana; The Linnean Society of London; and the Society for the Preservation of the Fauna of the British Empire.

The length of the foregoing list of positions held and honors received bears witness to the esteem in which he was held in the university and scientific world both in his native land and abroad; but it does nothing to picture the impress that he left on those who came to know him. An individual of striking appearance, six feet five inches high, bulky in proportion, with curly hair, black in early life but silvery gray in later years, one of his close associates has described him as a "genial giant." And he commanded attention in any gathering. Doubtless it was from his grandfather from northern Ireland that he inherited his keen sense of humor, his unusual charm as a conversationalist, and his readiness to speak out in no uncertain terms and in language all his own when occasion seemed to demand. The charming trait of hospitality was strong in him. Soon after he became Director of the Museum he found it convenient to eat his lunch in his back office. Before long he invited some of the members of the Museum staff to join him, for companionship, each bringing his own lunch box, and thermos of milk or coffee. This was the start of what came to be known as the "eateria," presided over by Charles Gilbert who came to the Museum after many years' service with the famous ornithologist William Brewster. Nothing pleased T. B. more than to serve some choice piece of game—wild duck, elk meat, or perhaps venison, from his own shooting trips, or some exotic such as palm grubs from Panama—cooked by Gilbert over an electric hot plate in the Director's office. The "eateria" became so widely known and so popular that by early 1942, when Gilbert died, the visitor's book contained records of nearly

21,000 luncheon guests, including visiting scientists from many parts of the world. T. B., with a few of us had the honor of being photographed by Life Magazine, in appropriate attitudes, lunching at the "eateria."

The conversation at these informal little gatherings ranged wide; it might settle on serious Museum affairs; it might be the reverse of serious. If the company suited, as often was the case, the talk might be of river or upland, of salmon or tarpon, of grouse or woodcock, of deer or ducks, for T. B. was one of the most skilled woodsmen I have known. He was blessed with great physical strength and endurance. In his younger days he could outwalk most men; could endure in a duck blind while I was shivering with cold; he had the patience in the woods of the proverbial Indian. His keenness of vision coupled with ability to focus on some particular vista, was such that his eye caught many a forest happening that most men would have missed. These natural gifts were of great help to him in the field, collecting snakes and lizards.

Another gift for which he was noted among his associates was a retentive and photographic memory. He was an omnivorous reader, and passed many sleepless hours at night—he suffered from insomnia—browsing in books of travel, adventure, biography and history, as well as in the most recent scientific publications. In this way he kept up-to-date on a wide range of world and literary events as well as in more strictly scientific fields. As a colleague has written "offhand he could give you the dates of any voyage of exploration, the name of the ship and the names of the naturalists, and the facts concerning the discovery of many species of animals. He was familiar with a great number, never forgot their names or appearance, their distribution or relationships."

Everything he ever learned or read or saw was stored away to be brought forth when needed. His photographic memory for the names and looks of animals of all sorts—of people too—was prodigious. In his own words, "I have only to see a specimen once, and the object, together with its label are photographed on my mind." Alexander Agassiz was the only scientist of my acquaintance whose zoological memory equaled his.

Incidents long past were little if any less vivid to him. In his later years when failing health curtailed his activities, this vividness of recollection found expression in *Naturalist at Large* published in 1943; in *That Vanishing Eden*, a word-picture of the Florida of his youth, published in 1944; in his *A Naturalist in Cuba* that followed in 1945, and in his *Naturalist's Scrapbook*, published in 1946, after his death.

With his peculiar gifts, coupled with his interest in all groups of animals and plants, it is not astonishing that Barbour was in his own words "by inclination an old-fashioned naturalist, many tell me perhaps the last of the breed. My colleagues prefer to know more and more about less and less, and so are infinitely more erudite than I." But if "erudition" includes breadth of information, as it ought, as well as grasp of detail, this self criticism had no basis, for T. B.'s knowledge of groups of animals other than those on which he himself worked was little short of encyclopedic, a constant source of astonishment to his fellow students. His familiarity, for example, with recent discoveries in vertebrate palaeontology is made evident to every visitor to the Museum by the beautiful mounts of fossil vertebrates, which are perhaps the most attractive feature of the public exhibits. His purchase and gift to the University of Florida of the so-called "Thomas Farm" site which is the only good collecting ground for Tertiary Mammals in eastern North America climaxed his earlier contributions in support of collecting trips to productive fossil beds elsewhere. He also kept up-to-date on developments in geologic opinion, especially as regards the West Indian island arcs and the Caribbean coasts. His connection with the Woods Hole Oceanographic Institution kept him in touch with the current researches in marine biology, in fact his last two scientific papers were on bathypelagic fishes. And his life-long interest in American archeology found expression in the numerous collections that he brought back from Central America to the Peabody Museum at Harvard.

Barbour's scientific papers total some four hundred; about half of them on reptiles. The Check List of North American Amphibians and Reptiles, of which he was co-author with Leonhard Stejneger, has gone through five editions; his *Birds of Cuba*

through two, as has also a popular book by him on Amphibians and Reptiles, elaborated from a series of lectures that he delivered in Boston in 1923, under the auspices of the Lowell Institute. By his own statement, writing came hard to him during his early years, as did formal lecturing. But in later life he mastered an easy and readable style, which found expression in the books of reminiscences that came from his pen during his last three years. These give a better picture of his rather unusual personality than this formal biography, even though I knew him intimately throughout his scientific career.

His co-workers in herpetology, ornithology and ichthyology are better fitted than I to estimate the values of his scientific writings. The following summaries of his work on amphibians and reptiles, on birds and on fishes are contributed, respectively, by Arthur Loveridge, by James L. Peters and by W. C. Schroeder, co-workers with him in the Museum of Comparative Zoölogy.

Amphibians and Reptiles. One result of Barbour's visit to the Indo-Australian Archipelago in 1906-1907, coupled with the appearance in 1909 of Dr. P. N. van Kampen's "De Zoo-geografie van den Indischen Archipel," was the added stimulus it gave to Barbour's interest in faunal distribution. This culminated in his major work: "A Contribution to the Zoogeography of the East Indian Islands," submitted for his doctorate in 1910, though not published until 1912. In this paper Barbour points out that neither geologists nor zoologists are as prone as formerly to regard the earth's crust as stabilized and quotes extensively to show that widely accepted theories regarding insular faunas in Polynesia have had to be relinquished in the face of accumulating evidence stemming from the more exhaustive collections of the recent past.

Approximately 80 pages of Barbour's thesis are devoted to an annotated listing of the 170 species he himself had gathered. 50 further pages deal with an island-by-island consideration of the composition of the insular faunas, and another 24 to the zoogeographical implications to which allusion has been already made. Then follows what at the time was probably the

most valuable aspect of the contribution, 17 large folding tables that revealed at a glance the presence or absence of the herpetofaunal elements as then recognized, in each of 49, mostly insular, areas of the Oriental-Australian regions.

Binomials rather than trinomials were employed exclusively for the 285 forms of reptiles and amphibians listed by Barbour in the companion volume "A Contribution to the Zoogeography of the West Indies," that appeared in 1914. This was in accordance with the view to which he adhered until 1937, that as insular forms could no longer interbreed they should be treated as full species. It was largely due to the enthusiasm Barbour brought to bear on speciation in the Antilles, as well as to the collections he amassed during almost annual visits to the archipelago, that the number of forms in his (1937) "Third List of Antillean Reptiles and Amphibians" shows an increase of 198* over those listed in 1914. No fewer than 42 of these were described by Barbour himself, and a further 33 in joint authorship with colleagues, chiefly Benjamin Shreve.

Barbour's final conclusions regarding dispersal in the West Indies did not change greatly in the intervening years. He regarded this homogenous fauna as having spread during periods when land bridges still united the islands of today. Those like Grenada, whose postulated connection was with South America, naturally exhibit a preponderance of forms with South American affinities, while the Jamaican and Cuban herpetofauna, in which Central American types predominate, were populated through direct land connection with that region.

Always keenly interested in Cuba, it was natural that Barbour's studies should crystalize in "The Herpetology of Cuba" which, in 1919, he brought out in coauthorship with Charles T. Ramsden. To naturalists and residents alike this has proved a most useful work and is likely to remain the standard reference book on the island's reptiles and amphibians for some time to come, though outdated today by Barbour's own discoveries. Indeed, shortly before his untimely death, he was talking of bringing out a new edition, as the 68 species dealt with in the

* The correct figures being 285 and 483 respectively.

work had grown to an even 100 during the thirty years that had elapsed since its appearance.

Logically enough this work led to Barbour's undertaking a revision of the intriguing little geckos comprising the genus *Sphaerodactylus*, eight of which he attributed to Cuba. Though 11 in all were described as new, the novelties he discovered in some other neotropical genera were even more numerous, *Eleutherodactylus* with 17 and *Anolis* with 30, the great majority still treated as valid by other herpetologists.

Anoles, indeed, were Barbour's favorite group and for many years he entertained the idea of revising the entire genus. In view of the magnitude of the undertaking, however, he eventually relinquished the plan, contenting himself with placing at the disposal of other workers the data on variation and synonymy he had accumulated during a quarter of a century. These notes were published in 1930 and 1934, respectively, under the titles of: "The Anoles. I. The Forms known to occur on the Neotropical Islands" and "II. The Mainland Species from Mexico Southward." Both appeared in the Bulletin of the Museum of Comparative Zoölogy.

In 1940 Barbour, in collaboration with Dr. A. F. Carr Jr., brought out a treatise dealing with the nine forms of "Antillean Terrapins" occurring in that region. The basis of this study was an exceptionally fine amount of material that Barbour had brought together at considerable expense over a period of many years. The paper, together with the magnificent colored plates that illustrate it, is likely to remain the standard work on *Pseudemys* of this area for many years to come.

Barbour's interest in Neotropical fauna is sharply emphasized by an analysis of the 274 reptiles and amphibians, representing 120 genera, described by him. No fewer than 172 were from the Antilles and tropical America, 42 from the Oriental (chiefly China and Indonesia) region, 8 from the Australian (all Solomons or New Guinea, 42 (33 in coauthorship) from the Ethiopian, and only 10 from North America.

And yet, for many Barbour will be remembered chiefly by his "Check List of North American Amphibians and Reptiles" that has been a boon to herpetologists in the United States and

Canada ever since the first edition appeared in joint authorship with Leonhard Stejneger in 1917; followed in 1923, 1933, 1939, and 1943 by subsequent editions. Despite dual authorship the appearance of these editions was largely due to the drive and enthusiasm of Barbour and his willingness to underwrite the cost. Like any other great undertaking the check list has had its critics. Doubtless subjection to scrutiny by a committee of the American Society of Ichthyologists and Herpetologists, would have resulted in more generally approved nomenclatorial treatment of some groups, and the elimination of certain departures from generally accepted procedure. No other country, however, has been so well served in the matter of its herpetofaunal list being produced in so pleasing a style or with such clarity of format, to say nothing of having it brought up to date with such frequency.

In 1923, at the Lowell Institute of Boston, Barbour delivered a series of popular lectures that, in somewhat expanded form, he subsequently published under the title "Reptiles and Amphibians, their Habits and Adaptations" (Houghton Mifflin Co., 1926). This popular and copiously illustrated volume found a ready sale which led to a revised edition being printed in 1943.

Very early in his career Barbour embarked on a search for unlabeled type material in the collection of the Museum of Comparative Zoölogy, and in after years stressed to his students and coworkers the importance of carefully designating types. These he endeavored to acquire for the Museum on every possible occasion. So rich and varied was the resulting assemblage that he eventually decided to publish a list of them for the benefit of herpetologists in other institutions. The first instalment, entitled "Typical Reptiles and Amphibians," appeared in the Bulletin of the Museum of Comparative Zoölogy in June, 1929, and in February, 1946, was followed by the posthumously published "First Supplement to Typical Reptiles and Amphibians." Together the two papers listed a total of 2,268 species or races, a reference collection of inestimable value to future workers in the field of taxonomy, and a monument to Barbour's foresight and generosity.

But, as has been emphasized elsewhere, the influence exer-

cised by Barbour on herpetology cannot be rightly assessed by his publications alone, for it operated in countless ways. To Barbour many a young herpetologist was indebted for making it possible to come to Cambridge and study the collections; others will remember with affection the advice and aid that enabled them to carry to fruition some long-cherished ambitions with regard to field work.

Birds. Although primarily a herpetologist, Barbour possessed a wide knowledge of birds, picked up in his usual effortless manner; not only from reading but from collecting, and the study of museum specimens.

He possessed a particularly good knowledge of Cuban birds as the result of his many visits to the island, and, in addition to some short papers, published two books on the birds of Cuba. The first entitled "The Birds of Cuba" was published as Memoir No. 6 of the Nuttall Ornithological Club and appeared in 1923. Some years later he felt that the "Birds of Cuba" was out of date and completely rewrote it, incorporating in "Cuban Ornithology" many of his more recent observations. This second book was likewise published by the Nuttall Ornithological Club as Memoir No. 9 and appeared in 1943.

It was his intimate knowledge of Cuba and its birds that led to the last of the really important ornithological discoveries to be made in the Americas. Hearing vague rumors of strange creatures in the great Zapata swamp in southern Cuba, he dispatched a collector to this region who secured three new genera and species of birds, a rail, a wren and a finch. These birds were described by Barbour in the Proceedings of the New England Zoölogical Club and subsequently he prepared a brief account of them for the Auk, with a colored plate of each drawn by Allan Brooks.

The birds, or groups of birds, that he found most interesting were those of zoogeographic significance, and the study of the wide-ranging cosmopolitan families he left to the specialists.

As a museum administrator he seldom let an opportunity pass to purchase desirable specimens, or even entire collections of birds that might be offered for sale, or for financing expeditions in the field where bird collecting might be expected to

be the primary purpose of the expedition, and he himself usually was accompanied by a good bird collector in the field.

Fishes. Throughout his career as a naturalist Thomas Barbour was keenly interested in fishes. His published papers in this field are relatively few when compared with the many dealing with reptiles, but his interest was great nevertheless. There were few naturalists who could better separate the wheat from the chaff than he and this is well demonstrated by the many rare and otherwise interesting specimens of fish with which he enriched our museum. Many of these were obtained during his earlier days when he visited the East Indies and Japan, at which time he scarcely could have acquired a wide knowledge of fishes, yet many of his specimens are still unique in the museum collection.

An example of his penchant for keeping hot on the trail of any specimen that appeared to be unusual or desirable occurred in 1928 when the Boston Herald pictured and gave an account of a large fish brought in by the schooner *Wanderer*, from Browns Bank. It developed that this specimen was on display in the window of a food store in Boston. Tom hung on the phone until he had convinced the owner that this fish was of far more value to science than it would be as food. A few hours later it reposed in the museum collection. It proved to be a specimen of *Taractes princeps* known only from Madeira and one of the biggest prizes ever taken on our side of the Atlantic.

His first paper on fishes, published in 1905, concerns the Bermudian fauna about which very little was then known. In his later years his interest centered in the Ceratioids, a bizarre group including many rare and little known species of the deep sea.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

- Amer. Nat. = American Naturalist
 Ann. N. Y. Acad. Sci. = Annals, New York Academy of Sciences
 Biol. Bull. = Biological Bulletin
 Bull. Antivenin Inst. Amer. = Bulletin, Antivenin Institute of America
 Bull. Boston Soc. Nat. Hist. = Bulletin, Boston Society of Natural History
 Bull. Mass. Audubon Soc. = Bulletin, Massachusetts Audubon Society
 Bull. Mus. Comp. Zool. = Bulletin, Museum of Comparative Zoölogy,
 Harvard University
 Bull. New England Mus. Nat. Hist. = Bulletin, New England Museum
 of Natural History
 Geogr. Rev. = Geographical Review
 Harv. Alumni Bull. = Harvard Alumni Bulletin
 Journ. Hered. = Journal of Heredity
 Journ. Mam. = Journal of Mammalogy
 Mass. Guernsey Bull. = Massachusetts Guernsey Bulletin
 Mem. Mus. Comp. Zool. = Memoirs, Museum of Comparative Zoölogy,
 Harvard University
 Mich. Sci. = Michigan Science
 Nat. Assoc. Audubon Soc. = National Association of Audubon Societies
 Nat. Geogr. Mag. = National Geographic Magazine
 Nat. Hist. = Natural History
 New Engl. Nat. = New England Naturalist
 N. Y. Zool. Soc. Bull. = New York Zoological Society Bulletin
 Occ. Pap. Boston Soc. Nat. Hist. = Occasional Papers, Boston Society of
 Natural History
 Occ. Pap. Mus. Zool. Univ. Mich. = Occasional Papers, Museum of
 Zoology, University of Michigan
 Proc. Acad. Nat. Sci. Phila. = Proceedings, Academy of Natural Sciences,
 Philadelphia
 Proc. Amer. Acad. Arts & Sci. = Proceedings, American Academy of
 Arts and Sciences
 Proc. Biol. Soc. Wash. = Proceedings, Biological Society of Washington
 Proc. Boston Soc. Nat. Hist. = Proceedings, Boston Society of Natural
 History
 Proc. New England Zool. Club = Proceedings, New England Zoological
 Club
 Proc. U. S. Nat. Mus. = Proceedings, United States National Museum
 Sci. Mo. = Scientific Monthly
 Zool. Jahr. = Zoologischen Jahrbücher

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Otto Folin

OTTO FOLIN

1867-1934

BY PHILIP ANDERSON SHAFFER

To honor Otto Folin on the twenty-fifth anniversary of his appointment as professor of biological chemistry in Harvard University, after two years as associate professor, a group of his colleagues early in 1934 arranged for the painting of his portrait by an artist of distinction and for its presentation at a gala dinner to be held in the fall to celebrate the event.

The death of Folin on October 25 changed that occasion to a memorial meeting held on November 23, at which addresses were made by several of his associates. The portrait by Pollak-Ottendorf was unveiled at that time and now hangs in the library of the Harvard Medical School. It shows in the background a simple instrument and two flasks, an emblem of the colorimetric methods Folin introduced in biochemistry and medicine. This portrait has come to be recognized by historians¹ as an emblem of progress in a field of which Folin was a pioneer. The portrait is reproduced with this memoir.

The address by Henry A. Christian, later printed in *Science*,² is a glowing tribute by a friend of many years and excerpts from it constitute a fitting introduction to this memoir.

"My part . . . is to speak of Dr. Folin from a dual point of view, that of an internist acknowledging the significant value to clinical medicine and surgery of his biochemical investigations, and that of a member of the medical faculty . . . paying a tribute to him as a stimulating teacher and leader, above all as a wise, helpful and beloved colleague.

"I speak with the authority of one who daily in my care of patients now for many years has utilized the methods that Dr. Folin perfected both for a better understanding of what ails sick humanity and as a guide to their therapeutic management; and of one who for twenty-seven years sat with him in faculty and committee meetings, somewhat bedeviled his peace and comfort when I used the laboratory over his head and above all in personal contacts learned to know the quality of his mind and the character of his personality.

"You, my younger colleagues and students, scarce can vision medicine without the methods of blood analysis perfected by Folin and his pupils and those inspired by Folin's own accomplishments, so completely have these micromethods of quantitative analysis become a factor integrated into the web and woof of the fabric of clinical medical and surgical lore.

"Not all of the methods have been the product of Folin's own hand or originated in Folin's laboratory, but it has been, however, from his own ingenious methods and the wisdom of his approach to important biochemical problems that has grown the whole range of the microchemical analyses of the blood and other body fluids which are daily in use in hundreds of hospitals and thousands of doctors' offices the world over. He was the recognized leader in this phase of clinical laboratory technique, and some of his own methods are perhaps always in use - - - (somewhere in the world).

"Folin is now a fine tradition in the Harvard Medical School, not alone to the faculty, but to the members of twenty-six classes of medical students - - -; his personality, his character, his widely critical attitude toward men and their investigations, his friendly helpfulness to others, the restraint of his spoken word not failing in clarity, his modesty, his sense of humor and other qualities have endeared him to us. Long will the memory of him remain a potent factor in our individual activities."

Tributes by other friends and associates will be cited as we attempt to unfold the growth of this modest, unassuming but determined personality who left warm and generous memories among those about him and whose research so interested and stimulated a number of his contemporaries that together he and they may be said to have inaugurated a new branch of biochemistry, now called quantitative clinical chemistry. A comment by one of his contemporaries whose interest in methods paralleled that of Folin and gave rise to many controversies between them and also to their friendship, is the following paragraph in a letter written by Stanley R. Benedict soon after Folin's death.

"One of the qualities which so impressed me in Folin, so rare among scientific workers, was the fact that he was able to drop out personalities when it came to a matter of difference of scientific opinion. I have known no one with whom it was possible to have such strenuous differences of opinion or view-

point in scientific work and have this not interfere one iota in the close personal friendship which lasted over more than twenty-five years."

Otto Knut Olof Folin was born on April 4, 1867, in the village of Åsheda, province of Småland, in southern Sweden. His ancestors were small landowners in that region. Of the eight children of his grandparents, Jonas and Maria Folin, landowners in comfortable circumstances, two were sons: Jonas became a minister and Nils Magnus, the father of Otto, was a tanner in the village of Åsheda.

About the year 1850 Nils Magnus Folin married Eva Olson, daughter of a small landowner, when she was sixteen years of age. Thirteen children were born to these parents; first twelve sons of whom Otto was the youngest, and last a daughter, Gertrud. Five of the sons died in infancy and three in early manhood. Only the eldest, Wilhelm, who died at the age of 18, and Otto had more than elementary schooling. Among their relatives were several ministers and a district judge, but only his mother seems to have been an inspiration to her youngest son.

It is said that the father, Nils Folin, had a good mind but was a poor manager and his business as a tanner did not prosper. Support of the family depended more and more upon the mother; fortunately she was a woman of ability and courage. Faced with the heavy toll of illness and death among her children she decided when still quite young to become a nurse. At intervals she attended three annual sessions in Gothenburg to take courses for licensure in nursing and midwifery—concerned meanwhile about the young children she had to leave at home. She was at that time quite poor and it took heroic courage to carry through her plans. . . . She became licensed as the official midwife of a large district, was interested in her work and was much liked in the community. In her country practice she found means of supporting her constantly increasing family. Because she was on twenty-four hour duty, the home life was somewhat casual, although Otto remembered that his mother at home was efficient

and cheerful. She had a wonderful constitution, a cheerful disposition and a wholesome philosophy of life which enabled her to carry on until her children were self-supporting. The boys lived at home and attended the local school until confirmed in the Lutheran Church at the age of fourteen and then were sent to a neighboring town to be apprenticed in various trades.

When it came the turn of her youngest son, the mother contrived to give him three years of schooling in the home of the Rev. Mr. Lange who conducted a private school for boys. There Otto was drilled in elementary mathematics and in German. Following his confirmation in the church he was apprenticed for only a few months. Then at the age of fifteen Otto was sent to the "great advantages of America," where two of his brothers and an aunt with her husband had already settled among groups of their countrymen in Minnesota.

The ticket for the voyage (in 1882) was provided by his brother Axel who lived at Stillwater, a lumber town on the St. Croix River. There our young immigrant began the struggle to support himself, working at the log-boom while dreaming of ways to continue his schooling and learning English. At that early age he appears to have acquired a settled determination to become an educated man and to seek a career of service in some field that might yield satisfaction and perhaps a measure of distinction in life. One discernible source of that ambition was the example of his mother.

To gain a knowledge of English more quickly young Otto soon went to a less-graded country school near the farm of his aunt and her husband with whom he lived and worked as a farm hand for a year or two. With that meager preparation he returned to Stillwater, living now and again with his brother while passing grade after grade in the grammar and high schools of that town. In six years he was able to meet the requirements of the combined eight-year course. During the school terms he found jobs in the town to provide board and sometimes lodging. His favorite job, then and later, seems to have been as night clerk in hotels, where he could study at night. Summers he spent in the harvest fields or on the St. Croix log

boom. In later years Axel told his niece, "Otto was never idle. He studied early and late . . . even while sawing logs he had a book open before him." At the age of 21 he graduated from high school in the class of 1888.

In the fall of that year Folin secured employment in Minneapolis which enabled him to enroll as a student at the University of Minnesota. Four years later he graduated in the class of 1892, receiving the B.S. degree. Evidence indicates that his scholastic record was good, that he "did more work in chemistry than most students in the scientific course" and "showed marked ability in English." In November 1890 he became a citizen of the United States. During his senior year he was an editor of the student paper "Ariel", and was known among fellow students as an advocate of democratic policies in student affairs.

When the University of Chicago opened in the autumn of 1892, Otto Folin was one of the Minnesota graduates of that year accepted for graduate studies. He chose chemistry as his major and physiology as his minor subject; a choice which may be regarded as a first indication of his intent to become a physiological chemist. He was fortunate in having gifted teachers to guide his basic preparation for a career in that subject; J. U. Neff was the professor of chemistry and Jacques Loeb the professor of physiology. Julius Stieglitz, then a junior member of Neff's staff, was Folin's faculty advisor and later directed his research for the dissertation.

During the first years of this period Folin's efforts were devoted to preliminary courses in several branches of chemistry and related subjects required of graduate candidates. About this time Stieglitz had undertaken with Lengfeld and others what proved to be a prolonged investigation of certain molecular rearrangements that occur when organic compounds containing halogen attached to nitrogen (brom-acet-amides) were treated with sodium methylate, yielding urethanes instead of the expected hydroxyl amines. The primary interest in the problem concerned the reaction mechanisms, a matter of theoretical chemistry. When Folin was ready to begin his research, the

topic assigned to him was a study of the products of this reaction with a series of such compounds. The data recorded in his thesis, "On Urethanes", published in the *American Chemical Journal* (1897), give evidence of extended experience in organic preparations, in the conduct of these reactions and analysis of the products, but include little consideration of the mechanisms involved or discussion of the conclusions that might be drawn.

The reason for these omissions in Folin's thesis became evident to this biographer on reading the memoir of Stieglitz written by W. A. Noyes for the National Academy of Sciences. Noyes cites (page 293) the research on this problem (with Lengfeld) as the "first work of importance done by Stieglitz at Chicago", and states that a logical concept of the mechanism of such reactions was possible only in the light of the idea of electronic bonds advanced by J. J. Thomson in 1897; and was first appreciated by Stieglitz (and Noyes independently) in 1901,—four years after Folin's thesis was written. So it appears that Folin had the not uncommon experience of drawing a thesis topic the implications of which he was unable to grasp because the approach to its theoretical aspects had not yet been discovered. Furthermore it may be admitted, as suggested by one of his friends who read a draft of this memoir, "theoretical chemistry was not Folin's dish."

After completing the writing of his thesis in August 1896 Folin went to Europe to begin his acquaintance with physiological chemistry, to which field he had decided to devote his life. The first year he spent in the laboratory of Olof Hammarsten at the University of Upsala, not far from the scene of his childhood. Here he examined the properties and composition of an hydrolysis product of a glyco-protein, mucin, from sub-maxillary glands, a so-called "animal gum." A short paper bearing this title, published in Hoppe-Seyler's *Zeitschrift* in 1897 was his first contribution to biochemistry.

During the summer of that year he worked in Salkowski's laboratory in Berlin, where he took up an analytical problem which was to hold his interest at intervals throughout his life.

and also be the subject of his last paper in 1934: "on the determination of uric acid."

The following year was devoted to studies in Kossel's laboratory at Marburg where he was again assigned an investigation of another hydrolysis product of protein present in "Witte's peptone." There he met A. P. Mathews but was a year ahead of Walter Jones and P. A. Levene, three of his American contemporaries who studied in Kossel's laboratory about that time. Hilding Berglund, a fellow countryman of Folin and later a co-worker in his laboratory at Harvard, refers to this period in the following paragraph of an obituary article on Folin published in a Swedish journal.³

"During his stay in Marburg, Folin started getting eager. He had applied his knowledge of organic chemistry to biological problems and used his German in thorough discussions on long Sunday hikes with other young doctors from Kossel's laboratory. His interests in the intermediary stages of protein metabolism were started here, and he never quite lost sight of this problem, although it was somewhat in the background due to other interests during the last ten years. There he also discovered a new practise in colorimetry used in the brewing industry, which was not without connection with the appearance of his first colorimetric method, that on the determination of creatinine."

Three papers reporting the data from these laboratory studies in Europe appeared promptly in Hoppe-Seyler's *Zeitschrift*; and he returned to Chicago, where he was awarded the Ph.D. degree in 1898.

Ready and anxious to begin an academic career in physiological chemistry, there seemed to be no opening for him. That subject was not then represented at the University of Chicago; in only a few universities and medical schools in this country had the need for its cultivation been recognized—usually by its assignment to instructors in physiology or pharmacology or medical chemistry. Only Yale had a department of physiological chemistry, established in 1882 by Chittenden. So Folin accepted employment as chemist in a commercial laboratory, to await a different kind of opportunity which arrived a year later.

It is appropriate at this point to note two durable personal associations Folin formed at Chicago and to record the memories of one of his friends from that period.

Laura Churchill Grant, a young girl of Canadian parentage whose family lived in St. Paul, Minnesota, after graduation at Vassar College entered the University of Chicago as a graduate student in mathematics and economics. She received the M.A. degree in economics in 1896. She and Folin were married three years later.

Dr. Leo Loeb was a friend of the Folins from their days in Chicago and on request has written me his memories of that relationship. The following excerpts portray Folin's personality.

"When I think of these many years during which we were friends, there are certain points that impressed me.

"Foremost perhaps was his imperturbable equanimity. He always showed an even temperament: I have never seen him discouraged. He tended a little towards the optimistic side in all his attitudes and judgments. He was rather quiet, somewhat slow and deliberate in speech. He was thoughtful and thorough in sizing up situations, and attempted to see various aspects of a question; he wanted to be just. I never saw him excited or wrought up. He looked at persons and events in a quiet, friendly, rather hopeful way. He saw the little conflicts which constitute so great a part of life, in particular of social life, the tricks played by one on the other in the pursuit of the competitive struggle. Yet he accepted it all with a kindly, humorous spirit, without bitterness, without sarcasm. To a certain extent he even extracted some enjoyment out of these games, which he watched, without exactly participating in them.

"His thoughts were essentially practical and realistic, but at the same time he was interested in the general principles underlying the surface appearances. He was very appreciative of good work of others and very encouraging in the expression of his appreciation; I had occasion to experience it in my own case. Yet he could be critical. And he did not hesitate to express his criticisms. So far as political-social questions are concerned, he was a liberal and a democrat, but not a radical; he was essentially independent in all he did and thought; but he always sympathized with those who were underprivileged and he wished that their condition might be improved by legitimate means.

"He was a devoted husband and father: he was physically strong and tried to take all possible burdens from his wife who lacked his physical strength and power of resistance.

"On the whole, he had a happy life and he was appreciative of what men and life had given him; and others appreciated him. He accomplished much in his life work as a teacher and investigator."

In the summer of 1899, Folin was offered an assistant professorship of chemistry at West Virginia University. With this position in sight, he and Laura Grant were married on September 11, 1899 and moved to Morgantown, West Virginia. In that small institution, perched on a hill overlooking the Monongahela River, Folin gave during one year a course in quantitative analysis and another in elementary physiological chemistry. The writer of this memoir had the good fortune to be a student in both courses. The text used in the second course, the first Dr. Folin had given, was a laboratory manual by F. G. Novy; Hammarsten's book (translated by Mandel) was used "for reference." Many of the exercises in Novy's volume were omitted; the margins of my copy bear notes "no microscope," "apparatus not available"; but the omissions were more than compensated by the spirit of inquiry aroused in several of the students who voluntarily worked overtime on small tasks they were encouraged to think of as "research problems."

In the spring of 1900 Folin was offered a new position as research biochemist at the McLean Hospital in the suburbs of Boston. Learning that he might bring an assistant he offered the writer that post, an appointment held for three memorable and profitable years, the memories of which will color the following account of the beginning of Folin's career at McLean.

Dr. Edward Cowles, Medical Superintendent of McLean Hospital (for the Insane) at Waverley, Massachusetts, decided to establish a research laboratory for physiological chemistry, hoping that studies by that approach might in time contribute to better understanding of mental diseases. Folin was invited to plan, equip and conduct that laboratory and to formulate his own program. He accepted promptly because he sensed it would provide opportunities to carry out the kind of studies

he thought would interest him most and which he felt qualified to undertake. That hope and expectation were realized; furthermore the environment and facilities were exceptionally fortunate and agreeable.

At that time physiological chemistry was a novelty in Boston. Soon after his arrival at McLean in October 1900, Folin went to the libraries in Cambridge and Boston, and to consult a few friends and new acquaintances especially at the Harvard Medical School on Boylston Street. Frequently his young assistant accompanied him. One of these early visits was to A. P. Mathews, then assistant to Professor Bowditch. In search of a volume located in the pathology library, Mathews took his visitors to call on Dr. Councilman. When Folin was introduced as the physiological chemist at McLean Hospital, Dr. Councilman asked in his brusque manner, "So, do you know Pfaff?" Folin had to admit that he had not yet made the acquaintance of Dr. Pfaff, a reply that dampened and almost closed the interview. For Pfaff since 1898 had been the instructor in both pharmacology and physiological chemistry at the Harvard Medical School, with a laboratory at the Massachusetts General Hospital. He was perhaps the only avowed representative of either of these subjects around Boston at the time of this incident. Four or five years later Alsberg (in 1904) and L. J. Henderson (in 1905) were appointed instructors in biochemistry at the Harvard Medical School.⁴

During Folin's first months at McLean while the laboratory tables were being installed and the apparatus secured—most of it imported from Germany—the "chief's" time was devoted to reading about metabolism, the chemical composition of urine, and the claims (mainly in French medical journals) of the presence in urine of insane patients of toxic substances thought to be concerned with their mental states. The first experiment undertaken, as the writer recalls, was an attempt to test the toxicity of normal and other urines, and of their known constituents separately by injection in rabbits. The only marked toxicity observed was that due to potassium and ammonium salts, effects already known. Folin was skeptical of the idea then harbored

by some French writers and by Halliburton and Mott in England, that "toxins" perhaps related to choline or other nitrogenous bases derived from nervous tissue could be a factor in mental disturbance; yet that idea was probably in Dr. Cowles' mind in creating the chemical laboratory, and the possibility was not wholly rejected by Folin. Finding no evidence for it in the preliminary experiments Folin gave up that approach.

He decided instead to study the protein metabolism of normal vs. mentally disturbed individuals by measuring as accurately and as completely as possible all of the known nitrogenous and other products excreted in the urine, hoping thereby first to learn the normal range of variation in the partition of the total nitrogen among the known products and residual fraction and then to consider possible abnormal variations. Although the primary purpose was ostensibly a search for abnormal features, they were presumably unrecognizable except by contrast with the normal patterns which were then unknown. To establish norms would alone be an important undertaking and progress could in any case be made toward that.

The first essential would be to devise more and better quantitative methods before any worthwhile surveys could be started. These were the considerations that led to Folin's interest in devising suitable quantitative methods for urine and blood analysis, an interest which held his attention for the rest of his life. The methods he developed enabled Folin, and following him, many others with even better methods to explore normal and abnormal features of metabolism with consequences not then foreseen.

In the only autobiographic note left by Folin he describes the sequence of his work in a simpler way. Among old papers found in his desk at the medical school after his death was the following signed document in his own handwriting (dated April 9, 1924), a copy of which was sent to the writer by Professor Harry C. Trimble.

"When I was appointed chemist to the McLean Hospital in 1900 it became my duty to do chemical research on problems bearing on mental diseases. As the pathologist wanted all the brain material I took to the field of metabolism.

"It was hopeless to try to find deviations from the normal in the metabolism of the insane without far more exact knowledge of the human waste products than was available. My immediate and comprehensive problem became, therefore, the chemistry of urine. I realized that by thus interpreting my duty to the hospital I could do work of more general interest. I probably also followed my taste, for I enjoyed the mere puzzle aspect which is always present when one tries to devise a new method.

"My papers on the Laws Governing the Chemical Composition of Urine and a Theory of Protein Metabolism (1904) will probably be considered my best; but the data for those papers came easily and naturally by the help of the new methods for the determination of urea, ammonia and creatinine which I had devised during the preceding three years.

"My later studies—Protein Metabolism from the Standpoint of Blood and Tissue Analysis—in the main represent attempts to pursue experimentally theoretical concepts which I had developed on the basis of urine analysis. My elucidation of amino acid absorption does not stand out well, partly because it was presented in the form of a series of short papers, and partly because Van Slyke soon came into the field with a method of his own. My "best work" in the field of blood and tissue analysis, aside from the methods developed, should be the work on uric acid, now in press. It will be the starting point for much new metabolism literature."

The first years at McLean were mainly devoted to devising and testing methods for determinations of urea, ammonia, uric acid, creatinine and creatin, sulfates and urine acidity. Each of course was a different problem presenting difficulties not at once fully recognized, requiring change and improvement as experience in their use revealed the defects. The first of Folin's colorimetric methods was that for creatinine, the application of a color reaction of that substance with picric acid noted by Jaffé many years earlier. Although other color reactions had been used long before to estimate biological products, such as Nessler's reagent for ammonia in water analysis, Folin's method for creatinine, using a more delicate and precise instrument for color comparison—the Duboscq colorimeter pictured in his portrait, is commonly regarded as the introduction of colorimetry into modern biochemical procedures. This method for creatinine

and his later similar adaptation of the Nessler reaction for ammonia in blood and urine are still standard procedures.

At intervals some fourteen papers describing methods he devised during this period were published.

In the meantime the methods were used in studies of metabolism of normal individuals and selected hospital patients, each on uniform diets of known composition; the data being the amounts of the above metabolic products excreted in the urine, the complete collection of which was carefully guarded.

At this point the investigations were interrupted temporarily by Folin's first illness. In June 1903 he entered the Massachusetts General Hospital where Dr. S. J. Mixter removed a tumor of the left parotid gland and found it necessary to cut the facial nerve, altering Folin's appearance permanently. His concern was greatly moderated by the opinions of pathologists, including especially that of Dr. Leo Loeb, that the tumor was benign. There was no recurrence, and until late in life Folin's health was good.

In 1904 a long paper giving results of the metabolism experiments appeared in two installments in the *American Journal of Insanity* (to please Dr. Cowles)—where it reposed almost unnoticed, perhaps because of its forbidding contents. It recites at length reviews of literature, plan and methods with 67 tables of data, followed by cautious though speculative interpretations largely negative as regards relation to mental states. The summary states that "From a constructive, positive point of view it must be admitted that (the experiments) teach very little that is tangible concerning mental diseases, except for the suggestion—that general paralysis may be associated—with some demonstrable metabolism disorders." (An example of the exception had been reported in a paper, "On Phosphate Metabolism" in the *American Journal of Physiology* in 1902.) This statement brought to a close Folin's attempt to discover metabolic evidence related to mental states. But in the course of this study he had secured methods and a plan of experiments that promised significant results of more general physiological

interest, as he explains in the last paragraph of the summary of this paper.

"From a general physiological point of view . . . the data should have considerable value . . . to throw light on the laws of the normal secretion of urine. . . . In the continuation of this work I expect to use the diet (here) described and in addition another—low-nitrogen diet—which I believe will be more adapted to bring out any metabolism characteristic that may occur"

The next papers in 1905 were those Folin referred to as perhaps his "best." They are still cited in texts and monographs as the "classic work of Folin" on principles of intermediary metabolism; and as he wrote "my later studies represent attempts to pursue experimentally concepts which I had developed on the basis of urine analysis."

The genesis of the data and arguments presented in these papers, as explained in a footnote in the second one, is worth recording here.

"For the opportunity to examine the urines given in table 1 (the starting point of all subsequent experiments recorded in this paper), I am indebted, on the one hand to Professor Bowditch, and on the other hand, to Dr. Ernest van Someren of Venice. Professor Bowditch kindly brought Dr. van Someren on a visit to this laboratory, and the latter while here consented to remain the guest of the McLean Hospital long enough to permit the collection of a series of consecutive twenty-four-hour quantities of urine. Dr. van Someren is known to many readers of this journal through his close associations with Mr. Horace Fletcher, a popular writer on the value of the thorough mastication of all kinds of food."

As Horace Fletcher stimulated Chittenden to undertake one of his well known researches, so did van Someren furnish the clue that gave added emphasis and new direction to the development of Folin's investigations. The level of van Someren's protein metabolism was found by Folin to be about a third of what was then considered "normal," a fact which led him to study the influence of low-protein diets on the urinary products of other normal subjects.

From the data on normal—high and low—protein diets

Folin deduced the "laws" governing the composition of urine and on these he built his "theory" of protein metabolism. His data showed a clear distinction between those products in urine independent of the amount of ingested protein (creatinine) and those dependent upon it (urea, etc.), a distinction between "endogenous" and "exogenous" metabolism, a conception which became generally accepted and was a guiding principle for several decades.

The prestige resulting from these papers, together with the spreading popularity of his methods of chemical analysis, doubtless led to the selection of Folin for appointment in 1907 as associate professor of biological chemistry and in 1909 as Hamilton Kuhn Professor at Harvard.

Before surveying briefly the continuation of Folin's researches at Harvard, it may be of interest to glimpse the environment of the laboratory when he entered it. One of his first graduate students and later assistant professor on his staff, Walter R. Bloor, has written his memories of that period at this author's request. The following quotations are excerpts from Dr. Bloor's letter.

"I came to Harvard primarily because I had heard of Folin as that new brand of chemist, the biological variety, and my training seemed to steer me into biological chemistry. Coming to Harvard Medical School I found the department in charge of Alsberg and Henderson. They said that a new man, Folin, had just been added to the staff, that he was upstairs and interested in research—a term which sounded very large and out of reach of my limited background. However, Alsberg took me up and introduced me and I was asked to come in anytime I felt like it. Alsberg got me going on preparations and Henderson tried to interest me in pH. I felt that I would like to know something about this research business, so I came to talk a good deal with Folin. Before long I was fortunately made first assistant.

"Folin was much interested in the medical students' laboratory and spent the whole time there when the class was in session. He did not circulate a lot but took one student or a group and worked intensively with them, doing a piece of research with the exercise they were working on. There were some distinguished students in those first groups; George Minot, W. W. Palmer, F. R. Rackemann. Joslin was around often. Henderson continued to lecture to students. He and Folin couldn't

agree on urine acidity; Folin was for titration as the best measure and Henderson was for pH. Dr. Emerson, a pediatrician, often came to work with Folin on fat in feces, and was enthusiastic about cabbage juice for babies—before vitamins were thought of. Joseph Pratt worked in Christian's laboratory and came to see Folin often. Langley Porter and another clinician were working in the laboratory.

"When I returned to Harvard (1914) there were a good many graduate students; Sumner, Doisy, McElroy, Youngbird, Pettibone. Fisk had graduated in medicine, was then an assistant. Rappleye, then a medical student, did a piece of original work, mostly biochemical, every year in school. Richard Bell and Richard Lyman (both physicians) each paid the other his salary and were active workers in the laboratory. Bell worked with Doisy and Briggs. (Lyman was a joint author with Folin in a number of papers of clinical interest). Folin used to bake the family bread on Saturday night and Sunday morning. His lunch was rolls and butter with coffee he boiled on the Kjeldahl still."

Besides those named by Bloor, others who were close associates of Folin in his own researches were W. Denis in the series of papers on "protein metabolism from the standpoint of blood and tissue analysis," H. Wu on "system of blood analysis," H. Berglund, A. Svedberg, Trimble, Marenzi and Malmros in later years.

One of the major contributions by Folin was to foster and encourage an interest in biochemical research among all who came within his range of personal contact: medical students, graduates, physicians from far and near, and visitors who came for his counsel. Among those named above three are Nobel laureates, and others have attained distinction comparable to his own. That is not to say that Folin's influence is discernible in their accomplishments; he expected individuals to grow of themselves and made no attempt to train them.

Of the many papers listed in Folin's bibliography most of them are short descriptions of analytical methods and their revisions, to which he devoted much of his time, probably because he "enjoyed the puzzle aspect." Also he had to defend and improve his inventions, for he soon had many competitors. It would serve no purpose now, at a much more advanced stage

of this methodology, to consider Folin's procedures for urine and blood analysis, except to note again his preference for measurements by colorimetry which he adapted successfully for many different substances. With improvements a number of his colorimetric methods are still in wide use. Their popularity is attested by the demand for repeated editions of the laboratory manual he published in 1916, the fifth edition of which was published in 1934. This volume also set the pattern for many mimeographed laboratory directions in medical schools and hospital laboratories everywhere.

Apart from his methods Folin developed concepts of intermediary metabolism already referred to as his theory of protein metabolism, based first merely on urine analysis. In 1912 he turned to a study of these problems by quantitative analysis of blood and tissues. It had been shown long before that food protein is digested in the intestine rather completely to amino acids, yet by ordinary methods no considerable concentrations of these products were to be found in portal blood. In consequence it had been supposed that the amino acids were during absorption re-synthesized to protein by the intestinal mucosa. Folin devised a simple method to determine fairly accurately the non-protein nitrogen of the blood, including urea and all other nitrogenous substances of small molecular weight such as free amino acids, ammonia, etc. It was this method, referred to as "blood n.p.n.," that was at once taken up by clinicians to measure the adequacy of kidney function.

With this method he was one of several to demonstrate that the free amino acids liberated from food protein in the intestine *are* present in portal and systemic blood, thereby disposing of the idea that resynthesis occurs in the mucosa. Next he undertook to follow the fate of absorbed amino acids when absorbed from blood by tissues.

With it Folin and Denis began a series of experiments to trace the fate of urea, and individual amino acids injected into the small intestine or intravenously in animals. About the same time D. D. Van Slyke, as Folin states in his brief autobiographic note quoted above, undertook similar experiments

which led to different conclusions. Folin concluded that all tissues absorb and deaminate the amino acids while Van Slyke believed from his data that the liver plays the dominant role in the deamination and conversion of the ammonia formed into urea. This issue was decided by Mann's results with animals after extirpation of the liver, when urea formation ceases. This conclusion was later confirmed and the enzymic mechanisms were discovered by which the liver performs this reaction.

In the course of this work Folin had hit upon phosphotungstic-phospho molybdic acid complexes as sensitive color-producing reagents, applicable to a number of substances, phenols, tyrosin, uric acid, glucose, etc., under appropriate conditions. Also he found that tungstic acid was a simple and effective way to precipitate completely at about neutral reaction all of the proteins of blood without adsorbing the non-protein constituents. These tools were the basis for the "system of blood analysis" developed from 1920 on with the collaboration of Hsien Wu. In tungstic acid filtrates, sugar, non-protein-nitrogen, urea, uric acid, creatinine and creatin, amino acids, chlorides and other substances could be determined by the sort of micro-methods Folin was an expert in devising. As Berglund states in his appreciative article about Folin, "This is the work that made Folin's contribution to medicine so important. No hospital laboratory today is unaffected by Folin's work."

Many others had by then joined in improving and extending accurate and sensitive micromethods for blood and tissue analysis and their wide application in both experimental studies and in routine clinical diagnosis and therapy. In this development Folin and a number of his co-workers continued to participate.

So rapid was the growth of information from quantitative clinical chemistry and its spreading use in biochemistry, physiology and medicine, that special treatises began to appear, special courses of instruction were provided and even special professions of practitioners of clinical chemistry had arisen and become organized. The monograph of Peters and Van Slyke, published in 1931, second edition 1947; references in texts to "the pioneer work of Folin, Benedict, Van Slyke, Myers and others (who)

initiated the present phase of quantitative clinical chemistry," (Hawk, Oser, Summerson, 1947); and the tribute paid at the organization meeting (1949) of the American Association of Clinical Chemists, "it was largely the result of the pioneer work of Benedict, Bloor, Folin, Van Slyke, and others, beginning at the turn of this century, that the science of clinical chemistry evolved as we know it," attest the influence of Folin's early work.

Let us pause here to relate incidents reflecting other aspects of Folin's life during his later years.

It is not surprising that he retained fond memories of his boyhood in Sweden and high regard for the land of his birth. After his return to America in 1898, he did not revisit Europe until the summer of 1913, when he spent a short time in England, gave a paper at the International Congress of Medicine (published in abstract in *Lancet*), visited his relatives in Sweden and attended the International Physiological Congress held that year in Groningen.

In 1918 the University of Lund, Sweden, awarded him an honorary M.D. degree, and shortly thereafter tendered him a professorship. Mrs. Folin tells that "this honor touched his feelings more deeply than any other which he received." She has described also their last visit together in 1932.

"His health was definitely failing but he wanted to see more of Sweden than he had been able to earlier. Traveling was difficult for him because he suffered great discomfort in his feet, due to his circulatory condition. Dr. Andrea Andreen-Svedberg kindly secured a good car for us, and our daughter, Teresa, who had just graduated from Johns Hopkins Medical School, had time to drive her father and me. In this way we saw in a leisurely manner a great deal of southern Sweden and visited Otto's sister Gertrud in Traryd and his brother Gustaf and family in Almhult. We also went into Norway and Denmark. Dr. Svedberg, Dr. Malmros, Professor Liljestrand, Professor Berglund and many other friends made our stay in Sweden a very happy experience."

Some of Folin's personal characteristics and tastes have already been touched upon. One was his liking for golf, a game

which he and the writer first tried to teach one another on the course at McLean. There neither succeeded; but after Folin went to Harvard and moved his home from Avon Street, Cambridge to 133 Buckminster Road, Brookline, he became a member of the Chestnut Hill Golf Club and soon became an ardent and fairly good golfer, able to coach his son to become an expert. Over many years golf was a hobby and diversion, doubtless of great benefit in keeping him physically fit. He also credited to one of his cronies in golf his discovery of "permutit" for the extraction of ammonia from urine, one of his clever methods.

No one of his associates knew Folin better at close range in the later period of his life than Dr. Harry C. Trimble. From the article² he wrote in memory of his chief we quote the following paragraph.

"To those who had the privilege of personal association with Folin he was always a modest, friendly and unassuming leader. Finding his chief delights in his family circle, his department at Harvard Medical School, and in the game of golf at which he excelled, Folin's daily routine was simple and regular. Every morning of the school year he was early at his laboratory. There the young and struggling worker in biochemistry, whether from his own or other departments, or other universities, would always find the door unlatched. To all who entered, sympathetic, judicial discussion and encouraging counsel were available. A quaint and always kindly humor made of every interview an occasion. Tall, erect and spare of frame, possessed of a simple, innate dignity, he was always a striking figure, whether in his short white laboratory coat, in the lecture room, or at public meetings of scientific societies."

Improvement of techniques remained Folin's pride and joy; creatinine and uric acid held his interest to the end. His last paper sent to the *Journal of Biological Chemistry* on June 7, 1934 describes a "Simple process for preparation of a correct uric acid reagent (and some comments)." Its concluding comments are prophetic: "This is presumably my last paper on the preparation of the uric acid reagent and I hope that the method for the determination of uric acid described last year is also final." At the end he adds, "the determination of uric acid represents probably the most complex reaction that we have in

the whole field of practical colorimetry." He knew that he had not finished that problem; but he gave it his first and last efforts.

A touching glimpse of Otto Folin in his last days is the following characteristic letter written a few days before his death. The plan for a dinner in his honor, mentioned in the opening paragraph of this sketch, included an invitation to this writer to be a speaker about Folin as a biochemist. I wrote him a note to say I had gladly accepted the invitation and to ask suggestions about the tone and content of my remarks—on what I knew would be for him a painful occasion. The reply is written in a firm hand and in his normal style, betraying no evidence of the physical weakness he must have felt.

October 21, 1934.

Dear Shaffer,

Your nice little note came yesterday. First of all I must tell you that I have been and still am in more or less trouble. We left suddenly our summer place and Mrs. Folin took me straight to the Brigham Hospital about September first. I stayed there for ten days and since that time I have been confined to the house, most of the time in bed. I have a nasty combination of cystitis and orchitis. It is not painful and I have no temperature any more but I am supposed to rest and to drink so much water as to make it impractical to go to the school. It is getting to be very tedious and I don't believe now that I shall be in good order even by November 16.

Cannon came in and told me the other day about the arrangements for that date. He said that Christian would speak about my contributions to medicine and yourself about my contributions to biochemistry. I am rather sorry for both of you as well as for myself.

A light tone would be desirable and, of course, would be less trying on me, but I shouldn't overdo it before an audience of New Englanders. I should think that a concise sketch of the practical biochemistry as it was when you and I began our work at McLean is called for since that condition served to give a trend to my work on methods. My portrait includes a colorimeter and a couple of volumetric flasks, and it might therefore fit in pretty well to say something about the introduction of colorimetry into biochemistry. This is about all I can think

of at the moment, but if I think of anything else perhaps I will write you again. I wish it were over!

It was extremely nice and friendly of you to take on this work for your old friend; there is no one else that would be anywhere nearly as acceptable to me.

With kindest regards to Mrs. S.

Sincerely,

Otto Folin

Four days later he died of a heart attack.

"There is an intimate and touching resemblance between the countryside in Smaland around Malilla, and a little town, Kearsage, in a corner of New Hampshire, where Folin had his summer home and where his place of burial is. A resting place has been found in the stony field among the tall pines for one who was an outstanding scientist, a grave and upright man, beloved by everybody who came in contact with him." (Berglund.)

Otto and Laura Folin had three children. Joanna, who died in 1912; Grant, now a businessman in Detroit; and Teresa, M.D. Johns Hopkins, now married to Dr. Jonathan E. Rhoads, Associate Professor of Surgery at the University of Pennsylvania.

Though Otto Folin lived to see only one grandchild, there are at present (1949) nine, the youngest of whom bears the name Otto Folin.

A little known activity of Folin was his connection with the Metropolitan Life Insurance Company, related to the writer by Mrs. Folin. Folin and S. R. Benedict were appointed members of a committee to draw up a plan to reorganize the biochemical laboratory; shortly thereafter Folin was asked to become Director of that Laboratory. Although the offer was very advantageous financially he did not accept because he preferred to remain at Harvard. But he and Benedict shared responsibility for its conduct until Dr. Blatherwick became the director in 1928, when Folin and Benedict were given the title "Consulting Biochemists," an appointment Folin held until his death. At that time Dr. A. S. Knight was the Medical Director of this company. The writer suspects that this influence of Folin and Benedict may have been one of the seeds that later bore fruit

in the general support of medical research by a number of insurance companies.

Folin was an active contributor to the *Journal of Biological Chemistry* in which most of his papers were published and of which he was one of the collaborators from its first volume, issued in 1905. He was a charter member of the American Society of Biological Chemists founded in 1906 and in 1909 was its third president. A regular attendant at its meetings, often enlivened by his discussions and quiet humor, he was greatly respected by its members. When the Journal was transferred to the Society in 1919, Folin was elected to its Editorial Committee, of which he was chairman for many years. A brief obituary appeared in the Journal and a resolution in his memory was recorded by the Society.

He was a member also of the Physiological Society, the Society for Pharmacology and Experimental Therapeutics, American Chemical Society, and Society for Experimental Biology and Medicine. He was elected a member of the National Academy of Sciences in 1916. In 1932 he was made a member of Kaiserlich Deutsche Akademie der Naturforscher.

He held the following honorary degrees: Sc.D. Washington University, 1915; Sc.D. University of Chicago, 1916; M.D. University of Lund, Sweden, 1918. Among the honors conferred on Folin the following tribute from the minutes of the Faculty of Medicine, Harvard University, December 7, 1934 should be included.

"We, members of the Faculty of Medicine of Harvard University, in meeting assembled, do herewith record our sorrow at the death of our colleague, Otto Folin, Hamilton Kuhn Professor of Biological Chemistry.

"Since 1907 Professor Folin has been a member of this Faculty, sharing in its deliberations and contributing to its discussions with the wisdom of a well-trained mind. Quietly, forcibly, with clarity of expression and touch of humor, was it his custom to share with us his opinions on matters under consideration by this Faculty; his words always carried weight; we valued his counsels; we knew the unselfishness of the man who spoke and the inherent modesty of one who never sought self-aggrandizement.

"Professor Folin, himself a recognized authority in his chosen field of biochemical investigation, was the ideal leader of an important department in this Medical School. Here he taught to class after class of medical students the facts and methods of a science that is of great importance to medicine and to them he gave the example of accuracy in methods of obtaining data to be used in the study and care of patients. To him came graduate pupils to be trained in investigation that they, in turn, might become teachers of students and seekers after greater knowledge of the chemical relationships of health and disease in man and animals. His pupils spread widely his influence into many laboratories for the betterment of mankind.

"We, his colleagues, pay tribute to the quality of the man and the significant importance of his contributions to biochemistry, particularly the development and application of methods of micro-analysis of body fluids and tissues. We recognize his leadership. We revere the memory of our colleague. Long will his influence be felt in the Harvard Medical School.

"What the Medical School is today is a mosaic of the lives of those who in the years since its founding have been of it: all of the brilliantly colored bits that make up this picture are attributes of the work and character of the men who have been members of its Faculty. Professor Folin has added gold and purple to the mosaic that we cherish.

"To Professor Folin's wife and children we tender our deep-felt sympathy in their sadness and sorrow."

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Professor H. C. Trimble provided a copy of a translation of Professor Berglund's article by Dr. Muus; a copy of the autobiographic manuscript by Otto Folin quoted in the text; and gave much other assistance including a careful preparation of Folin's bibliography.

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KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Jour. Insan. = American Journal of Insanity
 Amer. Jour. Physiol. = American Journal of Physiology
 Arch. Int. Med. = Archives of Internal Medicine
 Biochem. Jour. = Biochemical Journal
 British Med. Jour. = British Medical Journal
 Cleveland Med. Jour. = Cleveland Medical Journal
 Jour. Amer. Chem. Soc. = Journal of the American Chemical Society
 Jour. Amer. Med. Assoc. = Journal of the American Medical Association
 Jour. Biol. Chem. = Journal of Biological Chemistry
 Jour. Exper. Med. = Journal of Experimental Medicine
 Jour. Ind. Eng. Chem. = Journal of Industrial and Engineering Chemistry
 Jour. Phar. = Journal of Pharmacy
 New Eng. Med. Jour. = New England Medical Journal
 N. Y. Med. Jour. = New York Medical Journal
 Physiol. Rev. = Physiological Reviews
 Zeit. Physiol. Chem. = Hoppe-Seyler's Zeitschrift für Physiologische Chemie

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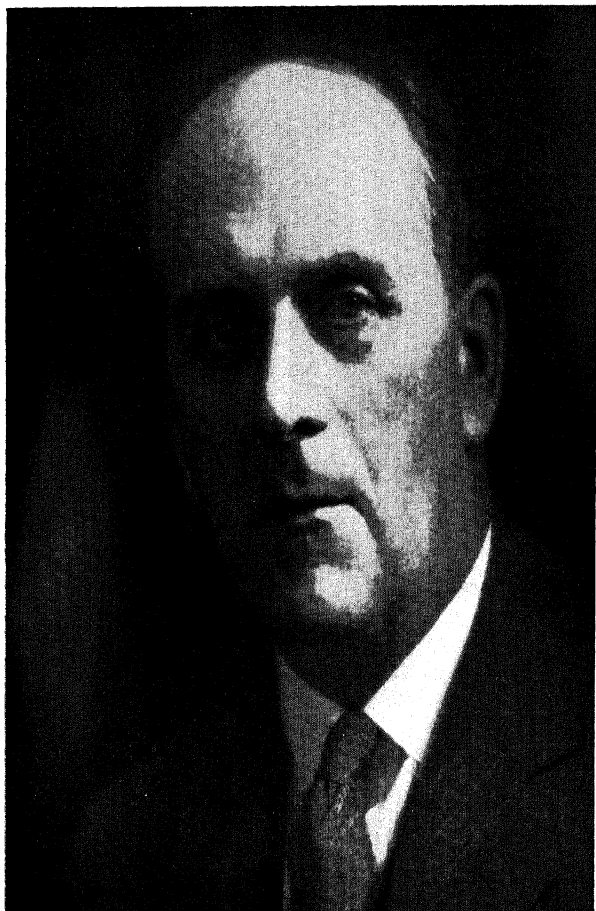
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Treat B. Johnson

TREAT BALDWIN JOHNSON *

1875-1947

BY HUBERT BRADFORD VICKERY

Yale University has been unusually fortunate among American centers of learning in the number of men who first attended as students and, after graduation, remained for the rest of their lives to become distinguished teachers and investigators. Treat Baldwin Johnson was one of these. He was graduated from the Sheffield Scientific School of Yale in 1898 and received the Ph.D. degree in organic chemistry in 1901. He had been a laboratory assistant during his graduate study and continued in this capacity until 1902 when he was appointed instructor. Promotion to the grade of assistant professor came in 1909 and to a full professorship in 1914. His outstanding attainments in science were recognized by the University in 1928 by an appointment as Sterling Professor of Chemistry, a position he held until his retirement in 1943. These dates are the brackets that enclose a lifetime of service to his University and to organic chemistry.

Johnson was born in Bethany, Connecticut, a small village situated among the hills a few miles northwest of New Haven, on March 29, 1875. His father, Dwight Lauren Johnson, who was born in the nearby town of Prospect in 1847, was a descendant through two successive Garry Johnsons of the daughter of a David Clark who served in New York during the Revolution. Johnson's mother was Harriet Adeline Baldwin who was born in Woodbridge in 1845, the descendant in the sixth generation of a Richard Baldwin who came from England in 1638 and settled in Milford, Connecticut, where he married Elizabeth Alsop of New Haven in 1642. The family names in successive generations, Mitchell, Hotchkiss, and Buckingham on

* The writer is most grateful to Mrs. Johnson who put family records and a file of bound reprints at his disposal. The bibliography is essentially as it was compiled for the Academy by Johnson himself in 1941 but has been completed from Chemical Abstracts. The kind assistance of Professor A. J. Hill in connection with records in the Sterling Chemistry Laboratory of Yale University is gratefully acknowledged.

Johnson's mother's side, and Doolittle, Hotchkiss, and Clark on his father's side, are still common in the vicinity of New Haven and belong to families of the highest repute. Johnson's genealogy is significant in that it goes far to explain his integrity, his singleness of purpose, his industry, and above all, his strong sense of personal obligation to serve his University and his fellow townsmen. These forebears were sound people, farmers and merchants for the most part with the heritage of their English yeoman ancestry, men and women of sturdy independence, thrifty, God-fearing and law-abiding citizens of the communities in which they lived. It is from such people that leaders spring.

Treat Johnson was the oldest of a family of three boys; a sister, the youngest of the family, died in infancy. The boys were brought up on their father's farm in Bethany, sharing in the daily chores and living the life of country boys everywhere. Treat attended the local ungraded country school where he soon attracted the interest of his teachers. The father of one of these teachers, a neighboring farmer, urged upon Johnson's father the desirability of giving his son an opportunity for a more advanced education. The decision was evidently a difficult one and was arrived at with characteristic Yankee uncommunicativeness. Late one September afternoon in 1890, after the boy had spent the day mowing with his uncle, his father appeared in the meadow and directed him to go to the house and get ready to go to Ansonia. He found that his mother had already packed his clothes and, a few hours later, he was deposited in Ansonia with orders to report to the high school the following morning. In this somewhat unceremonious way was Treat Johnson's higher education begun. His comment on the incident in later years was equally brief: "Some experience!"

Lest this story should give a false impression of the relationship between Treat Johnson and his father, it is of interest that, back in the early twenties, a brightly-painted ox neck-yoke hung over the fireplace in Johnson's summer cottage in Bethany. To visitors who inquired about it, Johnson would proudly ex-

plain that with that neck-yoke, his father had earned the money to send him to college.

Johnson's private life was a quiet one. He married Emma Estelle Amerman of Woodside, Long Island, in 1904 and for years occupied an apartment not far from the University. They had no children, and occasionally spent summers in travel or, in later years, in automobile trips to Maine or Nova Scotia. Early in the twenties, they built a small summer cottage on what had been his father's farm on Luke Hill in Bethany and, in 1928, erected a permanent home there. The view across the valley is one of the most beautiful in the state. Later, the cottage was occupied by a caretaker, and a small building was erected to house a private laboratory where an assistant helped with many personal investigations. In the item in "Who's Who", Johnson records himself with pardonable pride as "Director, Bethwood Research Laboratory, Bethany, Conn." and this, together with one other affiliation, stands at the head of a list of memberships that includes some of the most distinguished scientific societies in the world.

Light is shed upon Johnson's character by the order in which these apparently small matters are recorded. The other affiliation mentioned is that he was President of the Bethany Library Association, and it is clear that he regarded this at least as important as his professional connections. He had been active in the organization of the local library association and in the building of the library, and was also helpful in establishing an endowment fund for its support. In addition he served his native town as chairman of a committee which planned and built a new school and furnished one of the class rooms at his own expense. But what was, perhaps, his most significant civic service was the presentation in 1936 to the church he had attended as a boy of the Harriet Baldwin Memorial Church House in memory of his mother.

As an undergraduate in the Sheffield Scientific School of Yale University, Johnson came under the influence of H. L. Wheeler,¹ then well started on his career in the field of the synthesis of organic nitrogen compounds. Instruction in organic

¹ Elected to Academy 1909.

chemistry was also provided by W. J. Comstock, physical chemistry was taught by B. B. Boltwood² and analytical chemistry by H. L. Wells.³ The new Sheffield Chemical Laboratory had been first occupied in 1895, and the teaching and research were largely in the hands of a group of enthusiastic young men. R. H. Chittenden⁴ entered upon his duties as Director of the School in 1898 at the beginning of a period of reorganization and expansion of the facilities that soon placed the School in the forefront of American institutions devoted to the teaching of science.

Johnson received an appointment as a laboratory assistant in 1898, the year he obtained his undergraduate degree, and also the year in which, in collaboration with Wheeler, he published his first scientific paper. He entered the graduate school of the University in the fall and received the degree of Ph.D. in 1901 by which time he had been co-author with Wheeler of five more papers and sole author of one. In 1902, he was appointed as instructor in organic chemistry. Nine papers with Wheeler were published during the next two years but, from 1904 on, this collaboration became less and less frequent, and Johnson's name appears on most of the subsequent papers as senior author with a long succession of graduate students which, at the time of his retirement, had reached the astonishing number of ninety-four.

Johnson's early investigations dealt with the reactions of variously substituted ureas and thioureas and their use in the synthesis of hydantoins and thiohydantoins. In 1903, Wheeler and Merriam described a new synthesis of uracil and of thymine. The method consisted of the condensation of methylpseudothiourea with the sodium salt of formylacetic ester to give thiouracil and with formylpropionic ester to give thiothymine. The thiopyrimidines were easily desulfurized with acid and the procedure, as a whole, was far more elegant than that previously described by Fischer.

These substances had attracted attention as a result of the investigations of Kossel and his associates in Germany upon

² Elected to Academy 1911.

³ Elected to Academy 1903.

⁴ Elected to Academy 1890.

nucleic acid from which both pyrimidines had been obtained. In addition, Kossel and Neumann in 1894 had isolated from thymus nucleic acid a base of unknown structure which they named cytosine and, more recently, Kossel and Steudel had found the same base in nucleic acid from sturgeon testicles. They considered it to be in all likelihood a third pyrimidine. Wheeler and Johnson, in 1903, after consideration of various possibilities, prepared 2-amino-6-oxypyrimidine⁵ and 2-oxy-6-aminopyrimidine. They were able to show that the natural base described by Kossel was identical with the second of these isomeric substances. Although Kossel had assigned correct structural formulae to all three of the natural pyrimidines, it was Fischer's synthesis of uracil and thymine, confirmed by Wheeler and Merriam, and Wheeler and Johnson's synthesis of cytosine that completed the proof.

By an interesting coincidence, T. B. Osborne⁶ at the Connecticut Agricultural Experiment Station in New Haven, at about the same time, had obtained a new nucleic acid from wheat embryo⁷ and had demonstrated that the purines adenine and guanine were present in the preparation in equimolecular proportions. He had also been able to isolate uracil, but had failed to demonstrate the presence of cytosine. In order to compare the properties of their synthetic uracil with material from a natural source, Wheeler and Johnson asked Osborne for a sample of his preparation from wheat nucleic acid. Unfortunately, or perhaps fortunately as it turned out, Osborne had used up all of his material for analytical studies, but he was able to give them the mother liquors from which the uracil had been separated. Wheeler and Johnson at once looked for cytosine in this solution and, as a result of their experience with this substance, promptly succeeded in isolating it. The preparation was shown to be identical with their synthetic material as well as

⁵ To prevent confusion with titles in the bibliography, the numbering of the pyrimidine ring employed by Johnson has been retained. Present day nomenclature assigns Johnson's position 3 the number 1 and this substance would be 2-amino-4-oxypyrimidine.

⁶ Elected to Academy 1910.

⁷ Osborne, T. B., and Harris, I. F., *Ztschr. f. physiol. Chem.*, 36, 85 (1902).

with a preparation obtained from spleen by Levene⁸ and furnished by him for comparison. Cytosine was thus shown to be a natural base present in nucleic acid of both animal and plant origin. There is little doubt that Johnson's lifelong interest in pyrimidines was aroused by these successes.

Complete justification of his devotion came in 1935 when thiamine, the first of the vitamins of which the structure was elucidated, was shown by Williams⁹ to contain a substituted pyrimidine ring as a part of its structure and was subsequently successfully synthesized¹⁰ in at least three different laboratories practically simultaneously. It chanced that the exact degradation product of thiamine, 2,5-dimethyl-6-amino-pyrimidine had not been prepared either by Wheeler or by Johnson although several substances closely related to it had been synthesized. However, the existence of an extensive literature on the pyrimidines, much of it the work of Johnson and his students, enabled Williams to identify and synthesize the new substance with a minimum loss of time and effort.

Beginning in 1903, many of the students who worked with Johnson were assigned problems on the synthesis of various pyrimidines. The motivation of these investigations at the start, aside from the simple interest in the preparation of new compounds, was Johnson's concern with the problem of the composition and structure of nucleic acid. An example of this is furnished by the work (1908) on the action of fuming nitric acid on thymine and uracil. Thymine is converted into oxynitrohydrothymine, while uracil, under the same conditions, is not reduced but gives 5-nitrouracil, a substance which is almost insoluble in alcohol. Inasmuch as the thymine derivative is extremely soluble in alcohol, a method to separate these two substances was devised which could be applied to the separation of the mixture of uracil and thymine found among the decomposition products of nucleic acid.

Another important accomplishment of this period was the dis-

⁸ Elected to Academy 1916.

⁹ Williams, R. R., *J. Am. Chem. Soc.*, **57**, 229 (1935); elected to the Academy 1945.

¹⁰ Williams, R. R., and Cline, J. K., *J. Am. Chem. Soc.*, **58**, 1504 (1936).

covery in 1907 of a specific color test for uracil and cytosine that is not given by thymine. The test consists in the addition of bromine water followed by an excess of barium hydroxide. A purple or violet precipitate furnishes positive indication of the presence of one or both pyrimidines. The test can be so conducted as to be sensitive to a milligram of the substance. The mechanism of the reaction was worked out; both uracil and cytosine are converted by bromine to dibromohydrouracil which is decomposed in the presence of dilute alkali to isodialuric acid. This at once rearranges to dialuric acid with the loss of the elements of water. Both isodialuric acid and dialuric acid give blue precipitates with barium hydroxide. The blue compound, on being acidified and freed from barium, becomes colorless and the solution deposits crystals that were identified as alloxantin.

A further illustration of Johnson's interest in naturally occurring pyrimidines is furnished by the investigation of orotic acid. This substance was isolated from milk in 1905 by Biscaro and Belloni who assigned to it a structure containing a seven-membered ring. To Wheeler, Johnson and Johns in 1907, it appeared far more likely that the substance was a pyrimidine carboxylic acid, and they thereupon undertook the preparation of uracil-5-carboxylic acid. The properties of this substance were compared with those in the description of orotic acid given by the Italian workers. Although there were many points of resemblance, there were discrepancies in the melting points of the acid and of its derivatives. Wheeler, at the same time, prepared uracil-4-carboxylic acid (an ester of which had already been described by Müller) finding that this substance also differed from orotic acid as described by the Italian workers. Research on the problem was therefore dropped. However, Bachstetz in Italy in 1930, repeated the preparation of orotic acid from milk as well as the synthesis of uracil-4-carboxylic acid and was able to demonstrate that the natural material, when adequately purified, is identical with this pyrimidine. Johnson thereupon (1931, with Schroeder) returned to the study and was able to improve the synthesis so that uracil-4-carboxylic acid became a readily available substance. It was found that the corresponding aldehyde, which could be prepared

by methods developed in 1915 by Johnson and Cretcher, was easily oxidized to the acid. The identification of the synthetic substance with natural orotic acid supplied to them by Bachstetz was confirmed.

In 1911, perhaps in part because of the success of the investigations of Osborne at the Connecticut Agricultural Experiment Station in the protein field, Johnson's attention was turned to the consideration of proteins and amino acids, and especially to the hydantoins which may be regarded as derivatives of the amino acids and which are, at the same time, closely related to the pyrimidines. The first paper that specifically mentions proteins in the title (with Burnham) offered an interesting speculation on the possible presence of thiopolypeptide groupings in proteins as an explanation of that part of the sulfur of proteins not present as cystine. At that time, the presence of methionine in proteins had not been recognized. Other speculative possibilities were given in a later paper, and one of his students was set the task of preparing thioamides and thiopeptides to illustrate his views. The work on hydantoins led to the preparation of 3,5-dichlorotyrosine (Wheeler, Hoffmann and Johnson 1911), the analogue of the naturally occurring iodine compound which had been prepared synthetically by Wheeler and Jamieson in 1905; also to the preparation of thiotyrosine (with Brautlecht 1912) and of *N*-methyltyrosine (with Nicolet 1912). A particularly significant paper was that with O'Brien (1912) on the synthesis of phenylalanine. This procedure was a modification of the Erlenmeyer synthesis starting with hippuric acid whereby benzoylthiohydantoin is formed by condensation with potassium thiocyanate. The hydantoin is reduced with tin and hydrochloric acid and simultaneously hydrolyzed with the formation of phenylalanine. The synthesis required only three operations as compared with four or more by other published methods and employed readily available and cheap reagents. Glycine could also be used if hippuric acid were not available, since it can readily be converted into the same thiohydantoin and used for the synthesis.

Johnson's approach to protein chemistry was strictly that of an organic chemist interested in problems of organic structure.

A series of papers in 1915 dealt with the synthesis of *o*-tyrosine (with Scott) and with the problem of the effect of nitric acid on proteins (with Hill). This investigation led to the demonstration of the presence of nitrotyrosine in nitrated silk fibroin.

Work on the products of the destructive distillation of silk fibroin *in vacuo* (with Daschavsky 1919) showed that phenols and amines are produced. Accordingly, the decarboxylation of tyrosine was studied (1925) and an elegant method to accomplish this was developed. The amino acid was heated to 260° in solution in a mixture of diphenylamine and diphenylmethane. The mixture remained liquid when cooled and deposited tyramine in a yield of 95 per cent when diluted with benzene.

Two other syntheses of amino acids were described. In 1930 (with Law), he prepared thiothyronine; this is the sulfur-containing analogue of thyronine which, in turn, is produced by the deiodination of thyroxine and had been described by Harington. In 1939, the synthesis of α -aminopelargonic acid was described.

In 1922, a series of investigations directly related to biochemistry was begun as the result of arrangements with the National Tuberculosis Association whereby not only funds but liberal supplies of cultures of tuberculosis bacilli were made available for chemical investigation. The first of these studies (with Brown) led to the isolation of thymine and cytosine from the nucleic acid of the organism as well as to improved methods for the preparation of the specific nucleic acid. Later, (1925, with Coghill), 5-methylcytosine was demonstrated to be present in this nucleic acid. This success stimulated the elaboration of plans for a comprehensive study of the chemical composition of tuberculosis bacilli. The proteins of the cells were investigated by Coghill in 1926 and again by Renfrew in 1928. However, a new direction was given to the research by R. J. Anderson,¹¹ who joined the group in the Sterling Laboratory in 1926, and took over the investigation of the lipides of tuberculosis bacilli with results now widely known. For several years, Johnson continued investigation on tuberculosis bacilli as well as upon

¹¹ Elected to the Academy 1946.

other species of bacteria giving special attention to the nitrogenous compounds and carbohydrates; but the main supervision of the research upon bacteria was transferred to Anderson.

Several other investigations of substances of biochemical importance should be mentioned. The substance divicine was discovered by Ritthausen in 1879 in the form of a glucoside, vicine, in vetch seeds and was reinvestigated by him in 1899. It was a base, and the empirical formula published suggested to Johnson that it might be a pyrimidine. Johnson and Johns (1914) accordingly prepared 2,5-diamino-4,6-dioxypyrimidine and 4,5-diamino-2,6-dioxypyrimidine and compared their properties with the description of divicine given by Ritthausen. They inclined to the view that divicine is the 4,5-diamino-derivative. However, Levene at the same time prepared the natural substance and obtained a positive identification with 2,5-diamino-4,6-dioxypyrimidine.

A second accompanying glucoside isolated by Ritthausen and designated convicine was studied by Fisher and Johnson in 1932. The pyrimidine nucleus was shown to be 4-iminodialuric acid.

The general increase in interest in synthetic drugs in the third decade of the present century was reflected in Johnson's laboratory by such investigations as those which led, with Manske, to the synthesis of ephedrine (1929) and to the preparation of various hydantoins with hypnotic properties (with Herbst, 1932). However, by far the most important of these investigations, in view of the later results, was that with Lane in 1921. It was found that the antiseptic strength of resorcinols substituted in the 4-position with alkyl groups increased with the molecular weight of the *n*-alkyl group. The comparisons were made with respect to the antiseptic value of resorcinol itself and were carried as far as the *n*-butyl derivative. The fundamental method employed to prepare these substances was an original modification of the Gattermann synthesis; in the hands of others, the series was extended with the result that the germicidal effect was found to be maximal with the *n*-hexyl derivative. The outcome was the commercial production of hexylresorcinol under patent rights, and Johnson's share of the

profits was substantial. The business acumen he inherited from his Yankee forebears stood him in good stead, for this relationship led to the accumulation of a comfortable personal fortune.

Such investigations as those just mentioned did not in any way interfere with the fundamental research on pyrimidines carried on under Johnson's direction. On the contrary, these side excursions, for that is what they actually were, frequently represented the choice of a problem by a student or associate who had special interests, or were investigations carried out under grants of funds for research in special fields such as the chemistry of silk. This project was generously supported for many years by Cheney Brothers of South Manchester, Connecticut, and, later, by the Textile Foundation. Johnson had the disposal of many such fellowships over the years.

The pyrimidine work continued throughout Johnson's life. In 1941, the last year of his full activity in the laboratory, ten of the twelve papers published were contributions to pyrimidine chemistry. With the exception of a few years, notably in the mid-twenties when the major attention was given to the work on tuberculosis bacilli, from one to as many as ten papers on pyrimidine chemistry were sent off annually to the journals. The maximum output occurred in 1930 when twenty-six separate contributions were made; there were many years when from ten to twenty papers of which about one-third dealt with pyrimidines were submitted for publication.

Johnson was accustomed to numbering the papers on pyrimidines. Beginning with an early (1898) paper of Wheeler and with the Wheeler and Merriam paper of 1903 on the synthesis of uracil and thymine as number 2, the series was continued until 1944; Johnson's last published paper was number 182 in the pyrimidine series, and his complete bibliography numbers 358 titles.

In addition to the strictly academic research with graduate students and fellows, Johnson for years carried on personal investigations with the aid of paid assistants, in later years at the Bethwood Laboratory in Bethany, and the results of a number of these were patented, the rights being assigned to

one or another company for commercial exploitation. In all, some fifteen patents were issued to him in addition to several to his co-workers. Most of these patents dealt with such substances as thiazoles, diphenyl sulfides and sulfonic halides.

It is unfortunate that Johnson never took the time to summarize his investigations in pyrimidine chemistry in the form of a monograph. There are only a few publications of the review type. The earliest is his speech of acceptance of the Nichols Medal of the New York Section of the American Chemical Society in 1918 in which the history of pyrimidine chemistry is briefly given together with a clear account of the investigations at Yale up to that time. Far more extensive, although somewhat restricted in its scope, is the review of the amino and aminooxy derivatives of the pyrimidines which was prepared in 1933 in cooperation with Professor Dorothy A. Hahn of Mount Holyoke College. This article of more than one hundred pages was published in *Chemical Reviews*.

A broader treatment of the pyrimidines under the title "The Chemistry of the Pyrimidines, Purines and Nucleic Acids" was prepared as a chapter in the first edition of Gilman's "Organic Chemistry" in 1938.

Johnson was elected to the National Academy in 1919, the year after he received the Nichols Medal. He was a member of the National Research Council, the American Chemical Society, the American Society of Biological Chemists, the Connecticut Academy of Arts and Sciences, the American Institute of Chemists of which he was president 1926-1927, as well as of a number of other professional societies. He was a member of the Board of Trustees of the Sheffield Scientific School from 1920 to 1941, acting as secretary from 1923 to 1941 and as a member of the Executive Committee from 1921 to 1941. He was also a member of the Board of Trustees of Yale-in-China for many years.

As a teacher, Johnson was a profound influence in American chemistry. His lectures were clear, simple, abundantly illustrated, and scholarly. They were delivered in a manner that enabled the students to take copious notes and were in every way memorable. To be sure, the occasional unregenerate

student would keep score on the number of times the word "functions", or the even more typical Johnsonianism "functionate", was used in the course of the hour. The phrase "for example" is said, on one occasion, to have been used 127 times! To Johnson, complex organic molecules were often "constructions" and were so called because that is the way he thought of them. But, aside from a few appealing mannerisms of speech such as these, the matter was sound, solid organic chemistry. His students left Yale to go on to professional positions often of the highest eminence. They took with them the memory of a kindly, simple-mannered man whom they all regarded as a friend.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

- Am. Chem. J. = American Chemical Journal
 Am. Dyestuff Repr. = American Dyestuff Reporter
 Am. Rev. Tuberc. = American Review of Tuberculosis
 Ann. Coll. Symp. = Annual Colloid Symposium
 Berichte deutsch. chem. Gesell. = Berichte der deutschen chemischen Gesellschaft
 Biochem. Zeit. = Biochemische Zeitschrift
 Chem. Rev. = Chemical Reviews
 Ind. Eng. Chem. = Industrial and Engineering Chemistry
 J. Am. Chem. Soc. = Journal of the American Chemical Society
 J. Biol. Chem. = Journal of Biological Chemistry
 Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences
 Rec. Trav. chim. = Recueil des Travaux chimiques des Pays-Bas
 Zeit. f. physiol. Chem. = Zeitschrift für physiologische Chemie

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UNITED STATES PATENTS GRANTED
TO TREAT B. JOHNSON

Number of Patent	Date Granted	Title
1,743,083	Jan. 7, 1930	"Catechol Thiazoles and Process of Making the Same." (Assigned to Winthrop Chemical Company, Inc., New York, N. Y.)
1,970,656	Aug. 21, 1934	"Thiazole Compounds and Process of Producing the Same." (Assigned to Winthrop Chemical Company, Inc., New York City, N. Y.)
1,976,732	Oct. 16, 1934	"Phenolic Derivatives of Diaryl Sulphides." (Assigned to Sharp and Dohme, Inc., Philadelphia, Pa.)
2,011,582	Aug. 20, 1935	"Paramethyl Parahydroxy Diphenyl Sulphide and Method of Preparing the Same." (Assigned to Sharp and Dohme, Inc., Philadelphia, Pa.)
2,014,498	Sept. 17, 1935	"Halogenated Derivatives of Thiazole and Process of Preparing the Same." (Assigned to Winthrop Chemical Company, Inc., New York City, N. Y.)
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2,030,373	Feb. 11, 1936	"Derivatives of Thiazoles and Process of Preparing the Same." (Assigned to Winthrop Chemical Company, Inc., New York City, N. Y.)
2,065,808	Dec. 29, 1936	"Hydroxy Diphenyl Sulphide Derivatives." (Assigned to Sharp and Dohme, Inc., Philadelphia, Pa.)
2,146,744	Feb. 14, 1939	"Process of Preparing Sulphonyl Halides and Sulphonic Acids from Pseudothiourreas." (Assigned to Röhm and Haas Company, Philadelphia, Pa.)

TREAT BALDWIN JOHNSON—VICKERY

Number of Patent	Date Granted	Title
2,147,346	Feb. 14, 1939	"Sulphonic Halides." (Assigned to Röhm and Haas Company, Philadelphia, Pa.)
2,171,241	Aug. 29, 1939	"Silk." (Assigned to Textile Foundation, Washington, D. C.)
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Wm W. Hansen

WILLIAM WEBSTER HANSEN

1909-1949

BY FELIX BLOCH

A few weeks after his election to a membership in the National Academy of Sciences, William Webster Hansen died, on May 23, 1949, just before his fortieth birthday. A life wholly devoted to the pursuit of science had come to its premature end at the height of brilliant achievements.

Hansen was born in Fresno, California, on May 27, 1909. His paternal grandfather came from Denmark to America after fighting in the war in which Germany took Schleswig-Holstein from his country. His father had to go to work at the age of 12 and augmented his education by attending night schools, his interests tending towards mathematics and mechanics. He went into the hardware business in 1895.

As the first-born of two surviving sons, Hansen very early showed signs pointing in the direction in which his life's work was to develop. At the age of three he preferred to play with dry cells and various electrical toys, some of which he made himself; encouraged by his mother, it was the delight of his early boyhood to fill an ever-increasing part of the house with parts of electrical equipment and to test their functions. Soon his mathematical talents became apparent. The solution of arithmetical problems posed by his father was one of his favorite games; once having discovered it as a source of deep satisfaction, mathematics became an absorbing passion of his young mind.

By the time he entered high school he had acquired by himself a sufficient scientific knowledge to allow his graduation after two years, at the age of fourteen. Having decided to become an electrical engineer, but being too young to enter a university, he spent another year at the Fresno Technical High School before starting his college training.

Attracted by its famous school of electrical engineering, he

entered Stanford University at the age of sixteen. It was to become the main scene of his life's activity with ever stronger bonds, personal as well as professional, attaching him to the institution. The atmosphere of informality, combined with high scholastic standards, which he found here, was eminently suited to his temperament, and it is difficult to imagine a more ideal place for the further development of his particular genius than the one which he found at the very start of his higher education. With the exception of one subsequent year at the Fresno State College, all of his training was completed at Stanford. Here also he later accomplished the major part of his scientific work.

It is characteristic of our age of technology that Hansen, like many other physicists of his generation, received his first stimulus from the practical side of science and that he felt himself at first destined to become an engineer. While most others found this stage merely transitory and soon lost almost all interest in its applications, the applied aspect of science was always of vital importance in Hansen's life and strongly determined his course of action. At the same time, however, he was profoundly attracted towards problems of a more basic character than those offered to the engineering student. Shortly after his return to Stanford in 1928 he had the opportunity to work in the department of physics as a laboratory assistant; it was this first contact which decided him to devote his further work to physics and to leave the school of engineering.

The research in which he became immediately interested was an extensive study of the mechanism of x-ray excitation, carried out under the guidance of D. L. Webster; the fact that his participation was not long restricted to mere assistance but advanced to active collaboration is evidenced by the first appearance of his name in a paper published in the same year with Webster and two other collaborators, Clark and Yeatman.

Starting in 1929, Hansen did his graduate work at Stanford University during the subsequent three years; after the first year, his training had reached a level which allowed him to hold an instructorship at the department of physics for the

remainder of this period. At the same time he actively continued research on x-ray excitation. With quantum mechanics in its initial stage, there existed no reliable theory to predict the absolute primary yield of excitation and its dependence upon the energy of the exciting cathode rays and careful measurements were necessary to gain an insight into the underlying mechanism. The problem was complicated due to inevitable secondary processes, such as fluorescence and cathode-ray diffusion, which became particularly pronounced with thick targets and required a separate study. It took several more years before the problem could be satisfactorily solved and qualitatively understood and Hansen contributed substantially to the eventual attainment of this goal, deploying both his mathematical skill and his sound judgment in the construction of apparatus. During this period he had the good fortune to receive expert aid and encouragement from D. L. Webster, P. A. Ross and P. H. Kirkpatrick, who were staff members working in the same laboratory. His work in the field of x-rays, as well as his graduate work, came to its conclusion with his thesis on "Probabilities of K-electron ionization of silver by cathode rays" published in 1933 with Webster and Duveneck as co-authors.

After obtaining his Ph.D. in 1933, Hansen received a National Research Fellowship which enabled him to continue his studies for $1\frac{1}{2}$ years, most of this time being spent at the Massachusetts Institute of Technology and some of it at the University of Michigan. It is evident that he started his fellowship in a stage of transition, approaching complete scientific maturity and trying to find a field to which his talents would be particularly suited. Still under the influence of his previous work with x-rays he felt attracted towards more general problems of electromagnetic radiation; in his application for the fellowship he planned to work on complex spectra and on a relation between the intensities of line and continuous x-radiation.

Except for a few attempts in this direction, the specific program was, however, not followed. While at M.I.T., he became greatly interested in boundary value problems, stimulated par-

ticularly by P. M. Morse and his work in this field. In close relation to this new interest, the opening chords of the theme which he was to follow with the greatest success in later years are found in a memo entitled "Emission and Absorption of Radiation," and written at M.I.T. in 1933; although it deals manifestly with the radiation of atoms, no new results are claimed beyond those obtained earlier by Dirac. However, following a suggestion by E. U. Condon, the problem is treated in spherical coordinates instead of using the customary plane waves of radiation, with the advantage that this procedure leads to a natural separation between the various types of radiation, such as electric dipole, electric quadrupole, and magnetic dipole radiation. It is this very consideration of vector functions in more general coordinates which led Hansen later to the rhumbatron, his first and significant achievement in the field of microwaves.

Enriched by fresh stimuli and by numerous friendships established among his eastern colleagues, Hansen returned to Stanford in the autumn of 1934 as an Assistant Professor in the department of physics. He found here a somewhat changed attitude, reflecting the profound changes which physics had undergone in the previous years and meeting his own desire for new types of activity. While there were still many challenging problems to be solved in the investigation of x-rays and while these investigations were actively continued, particularly by Ross and Kirkpatrick, they had outgrown the stage of pioneering. Quantum mechanics had been firmly established and there was little doubt that it held the key for the proper interpretation of atomic phenomena. This, however, was not the case with regard to processes concerning the nucleus. Nuclear physics had just received a powerful impulse from Chadwick's discovery of the neutron and the successful experiments of Cockcroft and Walton in producing nuclear transmutations by artificially accelerated particles.

Plans were maturing at Stanford, as in many other places, to undertake research on nuclei by bombardment with particles accelerated to energies of the order of a million volts, and

Hansen, soon after his return, was deeply involved in these plans. In view of the experience with x-rays already existent in the department, it seemed natural to accelerate electrons, rather than protons and other heavy particles used in other laboratories, and particularly, to work with the artificial gamma rays thus produced, in close analogy to x-rays. It would lead too far to discuss the various stages reached and abandoned in the discussions of this program, with their occasionally rather ambitious scope. Nuclear and specifically neutron research was actually initiated shortly afterward, avoiding any major instrumental developments; the existing voltage of 200 kv was first used to obtain a weak neutron source through the d-d reaction, and a few years later a small cyclotron was constructed in close imitation of a prototype functioning at the University of California in Berkeley. While Hansen was always interested in the work and the results gradually developing from these modest beginnings, he was far more fascinated by the instrumental problems and the long-range possibilities which their solution promised to offer. Knowing his strength, he chose at that time the path which led him to become the applied physicist "par excellence." To the end of his life, however, pure and actually rather abstract research was held by him in almost exaggerated admiration; with typical modesty and humor he sometimes referred to his "platonic love for pure research."

In considering various schemes to accelerate electrons to high energy, Hansen preferred methods based upon the characteristics of rapidly varying fields to those using static or quasi-static voltages. With the latter he justly foresaw grave problems of insulation, and he would symbolize his preference by stating that it would be better to work with the vector potential than with the scalar potential. The idea which he decided to develop was suggested by experiments carried out by D. H. Sloane in Berkeley, where the voltage necessary for the acceleration of electrons was derived from a resonating coil. Adhering to the principle of electromagnetic resonance, it was evident

that with a given input power, the stronger accelerating fields would build up, the smaller the losses.

The problem of finding a resonating arrangement with low losses was solved by Hansen in an ingeniously simple fashion. Departing from the conventional circuit elements, he realized that the normal modes of vibration of the electromagnetic field in the interior of a closed conducting surface represent the ideal resonators. His above-mentioned earlier treatment of the radiation field in spherical coordinates allowed him immediately to discuss this idea quantitatively for the case of a sphere with the result that the losses in this case could indeed be expected to be considerably smaller than those of a coil and condenser arrangement, oscillating at the same frequency. With Webster taking a lively interest in this new possibility for electron acceleration, it was soon found, however, that the spherical shape would not be suitable for this purpose. On the other hand, it was clear that this particular shape of the "hohlraum" was not essential; one remembers that the normal modes of vibration in a cavity with reflecting walls were familiar to the physicist from the classical treatment of the black-body radiation by Rayleigh and Jeans who assumed it, for convenience's sake, to be rectangular. As a simple shape for which the acceleration of electrons should be possible, Hansen chose that of a flat cylinder. In the normal mode of lowest frequency the electric field is here parallel to the cylinder axis so that an electron with sufficient initial velocity, entering at a proper phase, would gain energy in traveling along this axis. Hansen's main efforts during the year 1936 and the early part of 1937 went into the construction of an electron accelerator, based upon the principles outlined above. While it served to test the theoretically predicted electrical characteristics, it never came to be used for the purpose for which it was designed.

In 1937 the brothers, Russell H. and Sigurd F. Varian, came to Stanford, deeply aware of the inevitably impending war and the urgent need for developing air defenses. As an old friend from his earlier years at Stanford, Russell Varian re-established close contact with Hansen, having realized that the resonating

cavities could be used in conjunction with a new source for short electromagnetic waves. It is sufficient to mention here the enormous later development of radar in order to emphasize the importance of this realization and the stimulus which it derived from forebodings of the coming war.

Hansen's original relatively large cavity was excited by vacuum tubes; it required a new principle to excite the considerably smaller cavities necessary as resonators for waves of 10-cm length. This was found by R. H. Varian in an ingenious arrangement, called the "klystron" where a first resonant cavity (the "buncher") produces modulation of velocity in an axial electron beam which excites a second cavity (the "catcher") reacting by feedback upon the first one. While it found its immediate use in the klystron, Hansen's resonant cavity, the "rhumbatron," rates as an independent invention of very great importance in the field of microwaves.

Hansen was promoted to Associate Professor of Physics in 1937. He devoted the following years until the end of 1940 principally to the development of the klystron in close collaboration with the brothers Varian as well as with D. L. Webster, J. R. Woodyard, and E. L. Ginzton. It was a period of intensive activity whose pace was set by the ever-darkening political horizon, and which required the simultaneous approach of widely different problems. A considerable part of Hansen's efforts was devoted to developing techniques of microwave measurements; besides, different forms of klystrons were developed and tested, and began to be used in a new system, later known as the Doppler radar, where the velocity of the object was observed by the frequency shift of the reflected radiation. As a sideline to these activities, Hansen found the time to publish several papers which exhibit his masterly skill in the mathematical treatment of radiation problems; in one of them (with J. R. Woodyard) a new principle of antenna design was formulated by showing that the total gain of an array is increased if the elements do not radiate exactly in phase in the direction of maximum transmission.

During this period, filled with intensive and successful work,

Hansen's life was also enriched from the personal side; in October 1938 he married Betsy Ross, the younger daughter of the late Professor P. A. Ross, thus establishing another firm bond with Stanford in general and with its department of physics in particular.

Hansen's work at Stanford came to a temporary end when, in the beginning of 1941, he and practically the whole group of his collaborators transferred their activities to the Research Laboratories of the Sperry Gyroscope Company in Garden City, Long Island. With patent interests in the klystron, this company had already supported the research at Stanford which was now to continue on a larger scale on its own sites. The development of the klystron was approaching the production stage, and with the war in Europe in its second year, the political developments clearly called for acceleration and expansion.

By the time of Pearl Harbor, Hansen was already a veteran of war research which he continued, with multiplied intensity and under various aspects, until the end of the war. With the concentration of microwave and radar research on the east coast and particularly in Cambridge, Hansen was soon a prominent figure among the many physicists and engineers who became engaged in the work and were mostly newcomers in the field. Equally versed in the methods and terminology of both, he was one of the first and most important links in the close connection between engineering and physics, which was responsible for the rapid development of radar. With his previous experience and clear insight in the principles of microwave techniques he was asked to deliver a series of lectures at M.I.T. and for a considerable period of time he willingly submitted to the strain of commuting between Garden City and Cambridge. In these lectures he touched upon almost all of the central problems, restricting himself to those topics which he knew to be of basic significance. Many of the leading scientists, engaged or about to engage in radar research, were among the audience and gratefully acknowledge the important stimulus received from his masterly exposition.

The development of the klystron and its use for microwave

techniques was continued in the Sperry laboratories, largely under the guidance of the men who formed the originating group at Stanford. Because of its high frequency stability and the fact that it could serve as an amplifier for microwave power, the significance of the klystron became ever more widely established. One of the important practical applications was its use as local oscillator in radar systems, and it has also become an invaluable tool for later microwave research.

While maintaining his active interest in these developments, Hansen's research also contributed to the nation's war effort in quite different directions. Much of his interest was centering on the possibilities of a Doppler radar system; in addition he worked on the problems of blind landing by microwaves and of electronic anti-aircraft directors. On one occasion the Sperry Gyroscope Company was asked to undertake work on superchargers, and without being able to go further, the directors resorted to submitting papers on the subject to Hansen's examination; it is typical of his versatility and quick grasp of essentials that Hansen immediately took up the problem and by going back to the basic principles was able, by his suggestions, to contribute substantially to the improvement of superchargers. Another important phase of his war research took place in the summer of 1943 when he worked for three months as consultant at the University of California on some of the problems which had arisen in the great project of atomic energy, carried out there under the Manhattan District.

Hansen unsparingly exerted himself under the great mental strain of all these activities; the truly heroic devotion to his tasks is highlighted by the fact that during these hectic war years he suffered several severe attacks of the illness of his lungs which, too soon, was to become fatal; even in the intervals of relative well being, it seriously reduced his physical strength. Due to the fact that he was of powerful build and that he never complained in his merciless self-discipline, it was hard, even for his close associates, to realize that he was working to the limit of his endurance.

Towards the end of the war, Hansen turned his interests to

problems of less immediate urgency. He ingeniously observed the mathematical analogy between the general problem of amplifiers and electrostatics through their common use of complex functions; the method allows the use of the electrolytic tank in the design of amplifiers and has become of practical value. Early in 1945, however, another interest arose in his mind which continued to absorb him almost completely for the remaining four years of his life.

It would seem that Hansen's work had carried him far from the ideas which he pursued ten years earlier, for accelerating electrons by means of microwave power, but he proved the truth of the French saying that one always returns to one's first loves. With an amount of power available from the multicavity magnetron, which would have seemed beyond the realm of possibilities before the war, the problem reappeared under an entirely different aspect. Hansen realized that high energies should be gained by electrons traveling through a wave guide which was driven by great power and which was designed to maintain the proper phase of the electric field along the path of the electron.

With many essential features of this linear electron accelerator already clear in his mind, Hansen returned in August 1945 to Stanford; during his leave of absence he had been promoted to a full professorship in 1942. Upon his return the formal steps towards the foundation of the Stanford Microwave Laboratory were taken with the aim of giving Hansen, as director, full facilities for the realization of his plans.

In the first few quiet months after the end of the war and before turning his energy completely to the development of the accelerator, Hansen worked on another program which had been brought to Stanford at that time. The author had become acquainted with some radio techniques through his war work, and planned to investigate the magnetic moment of atomic nuclei by a new method in which the moments were to induce radio signals capable of detection in a suitable receiving system. Asked for his expert help and advice, Hansen immediately became interested and his skill and sound judgment, together

with his many valuable suggestions, largely contributed to the fact that the method of "nuclear induction" was successfully demonstrated in the first days of 1946. At the same time Purcell and his collaborators at Harvard had independently discovered the same phenomenon and later improvements of the method by both groups have since made it an important new tool for the study of nuclear moments; but even in the elaborate present modifications there are still features which were originated by Hansen.

By adding to its staff and attracting visiting scientists, the Microwave Laboratory soon after its foundation started to grow rapidly and to populate densely the department of physics. Under Hansen's guidance the foremost aim became the actual construction of a linear electron accelerator and many problems, both of principle and of detail, had to be solved in its pursuit. The first pilot model was built primarily to test the type of disk-loaded wave guide which seemed most suitable for the purpose. It consists of a long cylindrical tube in which metallic disks with circular apertures are inserted. Each segment between two disks represents in principle a cavity resonator similar to the original rhumbatron, and the apertures provide a coupling between two successive segments. They are designed in such a way that the accelerating electric field acts always with the proper phase upon an electron traveling along the axis of the guide with its speed gradually approaching that of light. Starting with the mathematical theory of this structure and carrying it to high perfection, Hansen was concerned at the same time with the practical problem of finding simple methods for the production of a large number of segments with the necessary small tolerances. The other essential feature of the accelerator, a source of high microwave power, was incorporated in the first model in the form of a conventional magnetron. The first section was tested in the spring of 1947 and it proved beyond doubt the soundness of the underlying principles. In his report to the Office of Naval Research, under whose generous support the work was carried out, Hansen laconically announced: "We have accelerated electrons."

Through improvements during the subsequent years both the output current and the final electron energies were substantially augmented; delivering a peak current of several hundred microamperes at an energy up to 6 million volts, the first linear electron accelerator, 14 feet in length, is now operating in the department of physics and it is beginning to be used as a valuable research tool.

The final goal which Hansen set himself went, however, far beyond this first step. With the successes of the circular accelerating devices, the betatron and the synchrotron, in yielding electron energies of several hundred million volts, a linear accelerator was planned whose performance should equal and surpass them. This plan was not conceived in a mere ambition to reach the record of high energies; the fact that it avoids the high radiative losses of circular motion and that the electron beam shoots out at its end is a definite and characteristic advantage of the linear arrangement. To attain the desired energies of the order of a billion volts, an extremely large amount of pulsed synchronized microwave power had to be available and the expert collaborators of Hansen developed the bold scheme of developing the klystron to a point far beyond its previous power output, so that it could serve as the driving source. Encouraged by Hansen's faith in the seemingly extravagant proposal, the work was vigorously pursued and the operation of the first high-power klystron, shortly before his death, removed the last major doubt about the ultimate success of the large accelerator. This fervent wish of Hansen is now nearing its fulfillment in the new separate building of the Microwave Laboratory where the group of able men whom he had drawn to Stanford carries on the work, inspired by his genius.

It is befitting that the foregoing description of his scientific work should constitute the major portion of this biographical memoir, considering that Hansen subordinated all other aspects of his life to his devotion to science. Yet, this devotion became never more manifest than during the personal tragedy of his last years. The symptoms of an extensive bronchiectasis and fibrosis of the lung, dating back to his teens and causing periods

of severe illness during the war, became ever more menacing. Moreover, a deep shadow was cast over his life and that of his wife when their long-desired and only child died six weeks after his birth in the fall of 1947. Profoundly grieved, his physical abilities more and more reduced through fatigue and shortness of breath and in the full knowledge of his impending death, Hansen did not relax in the passionate devotion to his work to the last day. His condition was seriously aggravated through a trip to England in the fall of 1948, to which he forced himself in order to attend an international conference on accelerators. Barely able to move, he still visited the Microwave Laboratory in its new building during the spring of 1949, and at his bedside he gladly received visitors to discuss with them scientific matters, exhibiting the full clarity of his mind. Although it did not come without warning, his death came as a deep shock; his wife followed him a few months later.

With all its highly developed intellectual traits, Hansen's personality will be remembered by his friends as that of a warm human being. In unbounded generosity he gave advice and material aid to his fellow man whenever they were needed, making the acceptance of his gifts easy through his understanding and his natural tact. Rather shy and embarrassed in great social gatherings, he nevertheless was fond of good companionship and always ready for a joke. As an ardent individualist, there were few issues closer to his heart than those of human liberty; his feelings were particularly aroused over the attempts, witnessed in our time, to restrict the freedom of science.

Hansen's scientific work is characterized by its high degree of originality but through the brittle medium of physics one can also discern a strong artistic inclination. Although he did not allow himself the luxury to cultivate it in a more general sense, this inclination was otherwise apparent in his sensitivity to music; it found its strongest expression in his approach to scientific problems where he sought and found not only truth but a deep esthetic satisfaction.

Among the external signs of recognition for his outstanding achievements, Hansen received the Morris N. Liebmann Prize

of the Institute of Radio Engineers in January 1945 and the Presidential Certificate of Merit in March 1948. As the crowning honor of his career, he considered, however, his election to the National Academy in 1949, the announcement of which filled him with the last great joy of his short and rich life.

Acknowledgment

The author wishes to express his gratitude to the many relatives, friends and acquaintances of the late W. W. Hansen whom he interviewed while preparing this memoir. Special thanks are due to James L. Hansen for information on his brother's earlier years in Fresno, to Professor D. L. Webster and Mrs. Olive Webster for further valuable material, furnished to the author and to Dr. John R. Woodyard and Dr. E. L. Ginzton, Hansen's closest collaborators in his last years, for their comments concerning this period of his life.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

Amer. Phys. Teacher = American Physics Teacher

J. Appl. Phys. = Journal of Applied Physics

Phys. = Physics

Phys. Rev. = Physical Review

Proc. I. R. E. = Proceedings, Institute of Radio Engineers

Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences

Rev. Sci. Instr. = Review of Scientific Instruments

Terr. Mag. Atmos. Elec. = Terrestrial Magnetism and Atmospheric Electricity

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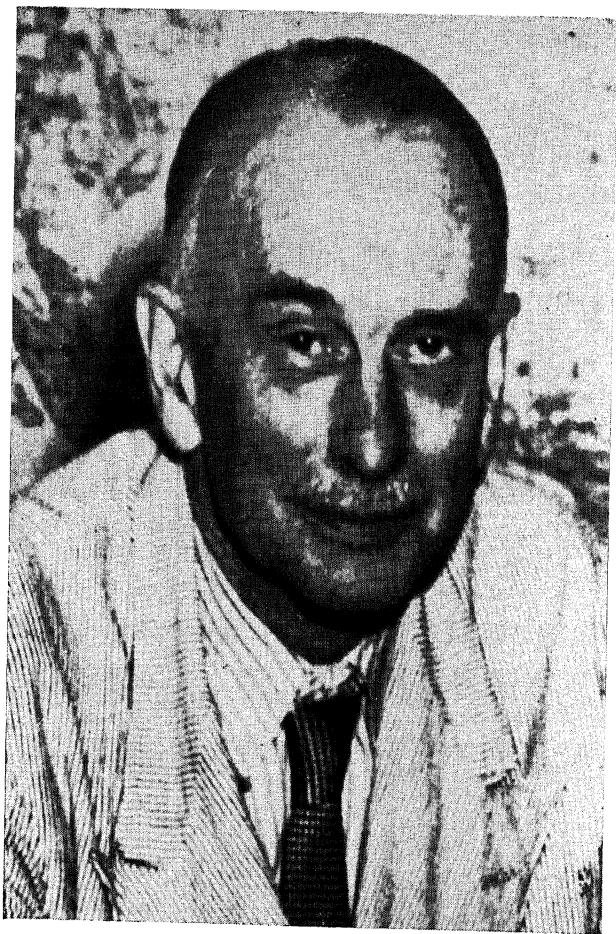
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Richard C. Tolman

RICHARD CHACE TOLMAN

1881-1948

BY JOHN G. KIRKWOOD, OLIVER R. WULF AND P. S. EPSTEIN

Richard Chace Tolman was born on March 4, 1881, in West Newton, Massachusetts, where he received his elementary and secondary school training in the Newton public schools. His father was a successful business man and his mother came from the Chace Quaker family of strong anti-slavery convictions. His mother's brother, Arnold Buffum Chace, for many years Chancellor of Brown University, exercised a major influence in Tolman's intellectual development.

After graduating from high school Tolman entered the Massachusetts Institute of Technology, from which he received the degree of Bachelor of Science in chemical engineering in 1903. He spent the following year in Germany, first studying at the Technische Hochschule in Charlottenburg, Berlin, and then in an industrial laboratory at Crefeld in northwest Germany. Returning to the Massachusetts Institute of Technology for graduate work he became Research Assistant in Theoretical Chemistry (1907-1909) and Research Associate in Physical Chemistry (1909-1910). During this period he came under the stimulating influence of Arthur A. Noyes, who pioneered the new science of physical chemistry in the United States. After receiving his Ph. D. in 1910, he served as Instructor in Physical Chemistry at the University of Michigan, 1910-1911, as Assistant Professor of Physical Chemistry at the University of Cincinnati, 1911-1912, and as Assistant Professor of Physical Chemistry at the University of California, 1912-1916. In 1916 he was appointed Professor of Physical Chemistry in the University of Illinois, where he remained for two years.

During World War I, Tolman served as Chief of the Dispersoid Section of the Chemical Warfare Service with the rank of Major. At the close of the war he remained in government service for three years, holding the posts of Associate Director and Director of the Fixed Nitrogen Research Laboratory of the

Department of Agriculture. In 1922 he was appointed Professor of Physical Chemistry and Mathematical Physics in the California Institute of Technology. Here he renewed his association with his friend and teacher A. A. Noyes, who had in the meantime come to California to participate in the founding and development of the new Institute. As Dean of Graduate Studies, a post which he held until shortly before his death, Tolman played an important part in the administration of the Institute for many years.

At the California Institute, Tolman began a long and productive phase of his scientific career. During this period he published many articles reporting the results of his experimental and theoretical investigations in physics and physical chemistry. He wrote four authoritative books, two on Statistical Mechanics (1927, 1938) and two on the Theory of Relativity (1917, 1934). His lectures in statistical mechanics, thermodynamics, and relativity were distinguished by their clarity, precision, and deep insight into the subjects. In recognition of his scientific achievements Tolman received many honors and academic distinctions. These included membership in the National Academy of Sciences, the American Philosophical Society, and the honorary degree of Doctor of Science from Princeton University in 1942.

Tolman was again called to national service in 1940. As Vice-Chairman of the National Defense Research Committee and Chairman of the Armor and Ordnance Division of that Committee, he played an important role in the organization and administration of the vast scientific program of the nation during World War II. As scientific adviser to General Groves, he was closely associated with the development of the atomic bomb. At the conclusion of the war, he contributed to the effort to solve the problems of international control of nuclear energy as scientific adviser to the United States representative on the United Nations Atomic Energy Commission. His distinguished service to the nation and to the world during World War II was recognized by the award of the United States Medal for Merit and the Order of the British Empire.

His war and postwar duties at an end, Tolman returned to

Pasadena in 1947 to resume his scientific work. After a tranquil and productive year, in which he completed several important papers on the thermodynamics of surface phases, he died on September 5, 1948, following a cerebral hemorrhage suffered without warning three weeks earlier.

Tolman's scientific interests were wide and varied. His rigorous and logical approach to scientific problems was reflected in the clarity of his writings and in the profundity of his physical insight which they reveal. His scientific legacy is unquestionably impressive and of permanent value. Tolman's first important contribution to science was the demonstration of the effect of centrifugal acceleration on the electromotive force of galvanic cells in experiments carried out at the Massachusetts Institute of Technology. Although this work grew out of the interest in applications of thermodynamics to chemistry characteristic of the time, it is worth noticing that the first paper contained both a thermodynamic and a kinetic derivation of the effect. Thus even these early investigations show the awakening of Tolman's interest in statistical mechanics, and foreshadow the later trends of his scientific thought in which the development of statistical mechanics played such an important role. It was precisely his deep understanding of the kinetic mechanism of the phenomenon that suggested to Tolman that not only the centrifugal acceleration of rotating conductors can produce an electromotive force (this fact was not new) but also the angular acceleration. In this way was discovered a new and more powerful method for measuring the inertia of the carrier particles of the electric current. At the University of California he performed in collaboration with Stewart the famous experiment demonstrating by this method the inertia of electrons in metals. The measurements were refined and extended in accuracy at the Fixed Nitrogen Research Laboratory in collaboration with Sebastian Karrer and E. W. Guernsey and at the California Institute of Technology in joint work with Mott-Smith. At the close of the article in which the results of the work at the Fixed Nitrogen Research Laboratory were described there is an acknowledgment characteristic of Tolman. .The

authors, after expressing their thanks to three sources that had given support to the work, add that they desire "in particular to express to the Government their appreciation of the policy of encouraging the staff of a government laboratory to devote a portion of their time and facilities to the investigation of fundamental scientific questions which have no immediate bearing on the main problem of the laboratory. It is believed that such a liberal policy is of great importance in maintaining a proper scientific attitude on the part of the staff of a research laboratory."

Other work in his early years at the California Institute of Technology consisted in many important theoretical and experimental contributions to the solution of problems of chemical kinetics. His papers in this field included a treatment of the problem of rates of molecular activation adequate to explain observed rates of reaction. They particularly clarified the meaning of the energy of activation. Important to this treatment was the behavior of monomolecular reactions of which no quite satisfactory example had been known. He took great interest therefore in the researches of Farrington Daniels and his colleagues on the decomposition of nitrogen pentoxide, which uncovered the fact that this now famous and much studied reaction, over a wide range of conditions, maintains its apparently monomolecular course. To account physically for the rates of molecular activation indicated by the observed rates of reaction presented considerable difficulty and the possible role of radiation in this process was much discussed. Several of Tolman's theoretical contributions concerned the analysis and development of the radiation theory of chemical reaction rate. It was in large measure due to this clarification that the inadequacy of the radiation hypothesis became quite clear.

These papers on chemical reaction rate pointed the way to further experimental researches, notably those of Daniels and of Tolman himself with his students in the years that followed. Particularly significant was further work on the nitrogen pentoxide decomposition with H. C. Ramsperger in which this reaction was studied at moderately low, and finally at very low,

pressure where activation depending on collisional processes should occur at a much reduced rate. Under the latter conditions a falling off in the first order rate of this reaction was discovered. He also gave a theoretical treatment of the temperature coefficient of photochemical reaction. In 1932 there was published the results of a unique experimental study, with P. D. Brass, of the thermal reaction rate of an extremely rapid reaction, the dissociation of nitrogen tetroxide.

In his monographs on statistical mechanics, Tolman gave the most searching analysis of the foundations of this important field since the work of Willard Gibbs. In many papers on the subject, he added to the structure of statistical mechanics. He was influenced by the work of P. and A. Ehrenfest in placing emphasis on the necessity of distinguishing between fine-grained and coarse-grained probability densities in the definition of entropy and in the proof of the Boltzmann H-theorem. Tolman's interests in statistical mechanics and thermodynamics went hand in hand. Shortly before his death, he had completed an important extension of the Gibbs theory of surface phases.

Tolman's early interest in the theory of relativity, which he shared with his friend G. N. Lewis, grew with the years. In 1917 he published a book on special relativity which made this important subject available to the English speaking reader. Later he became interested in the cosmological implications of general relativity. In a series of papers he developed important theoretical results which helped to establish the theory of the expanding universe. This work culminated in his monograph on relativistic thermodynamics where his results were summarized and extended. The consequences of this theory and the interpretation of observational results in terms of it were worked out by Tolman in collaboration with E. P. Hubble.

Tolman was a man of broad intellectual interests which extended far beyond his field of specialization. Through his wife, Ruth Sherman, whom he married in 1924, he developed an interest in psychology and in the social sciences. He was deeply concerned with the philosophical, cultural, and social

implications of science. His wisdom and humanism are well expressed in the following quotation from an address which he delivered at Brown University in 1947 :

“It is my faith that the ethical insight and scientific intelligence of man are such that the control of evil is possible. I am sure that humanity will continue to encounter great troubles, but I do not think that civilization will destroy itself. To surmount our troubles, we shall need courage, and patience, and clarity of thought, and sincerity in the advocacy of fair and reasonable courses of action. For these virtues we may pray, each in his own fashion.”

In the death of Richard Tolman, many have lost a wise and generous friend. The nation and the world have lost a distinguished scientist, a profound scholar, a great man.

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

- Astrophys. Jour. = Astrophysical Journal
Bull. Amer. Math. Soc. = Bulletin of the American Mathematical Society
Calif. Inst. For. = California Institute Forum
Chem. Metal. Eng. = Chemical and Metallurgical Engineering
Gen. Elec. Rev. = General Electric Review
Jour. Amer. Chem. Soc. = Journal of the American Chemical Society
Jour. Chem. Phys. = Journal of Chemical Physics
Jour. Frank. Inst. = Journal of the Franklin Institute
Jour. Infect. Dis. = Journal of Infectious Diseases
Jour. Opt. Soc. Amer. = Journal of the Optical Society of America
Jour. Proc. Ad. Assoc. Amer. Univ. = Journal of Proceedings and
Addresses of the Association of American Universities
Phil. Mag. = Philosophical Magazine
Phys. Rev. = Physical Review
Proc. Amer. Acad. Arts Sci. = Proceedings of the American Academy of
Arts and Sciences
Proc. Nat. Acad. Sci. = Proceedings of the National Academy of Sciences
Rev. Mod. Phys. = Reviews of Modern Physics
Rev. Sci. Instru. = Review of Scientific Instruments
Sci. Mo. = Scientific Monthly
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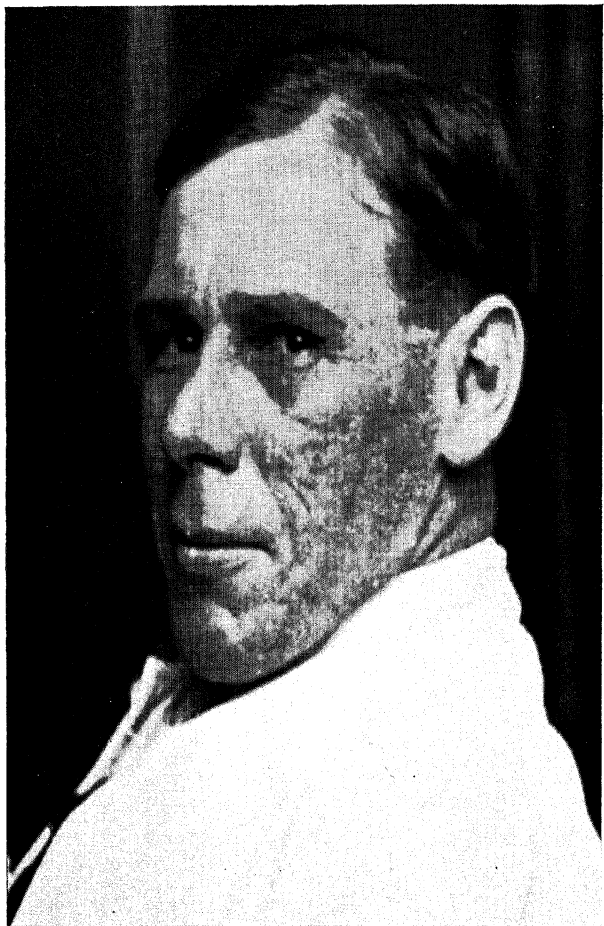
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Stanley R. Benedict.

STANLEY ROSSITER BENEDICT

1884-1936

BY ELMER VERNER MC COLLUM

It is not possible to give an accurate account of the scientific work of Stanley Benedict without at the same time discussing the parallel history of the researches of Otto Folin. The objectives of their researches were identical, and notwithstanding the difference in their ages, their years of creative work nearly coincided. The lives of both were characterized by sustained endeavor to discover truth. It is the merit of these two men that they succeeded, through many years of intensive investigations, in devising and refining analytical procedures for the determination of minute amounts of the principal non-protein constituents of blood and urine so that, for the first time, chemical analysis became a highly useful technic for the discovery of the chemical processes in the normal functioning of the body. These new methods also advanced to an astonishing degree the effectiveness of studies of the chemistry of pathological metabolism. In the entire history of biological chemistry no two individuals excelled these men in achievement. In the application of chemistry to the solution of biological problems, their work opened a new era, so that, in respect to knowledge of metabolism, historians will discuss the state of this department of science as it existed before and after Folin and Benedict.

Stanley R. Benedict was born in Cincinnati, Ohio, March 17, 1884, the son of Professor Wayland Richardson Benedict and Anne Kendrick Benedict and his death occurred on December 21, 1936. Stanley's father was professor of philosophy and psychology at the University of Cincinnati. His mother was a teacher and writer. She contributed stories to *The Outlook*, *Independent*, *Examiner*, etc. His maternal grandfather, A. C. Kendrick, was professor of Greek, Hebrew and Sanskrit at the University of Rochester, and a member of the committee for the revision of the King James Version of the Bible.

Stanley grew up in Cincinnati. He was next to the young-

est of six children. A sister, Dr. Mary K. Benedict, was at one time president of Sweet Briar College, but gave up educational work for the practice of medicine in New Haven, Connecticut. In a personal communication she told the writer of this memoir that at home the Benedicts lived in an atmosphere of intellectual inquiry and discussion, and that the family gathered every evening for a short time to listen to reading aloud by the father. They listened to philosophical theory, poetry, Dickens, Uncle Remus, and many more authors.

Stanley was educated in the public schools in Cincinnati, and at the University there. As a boy he planned to practice medicine, and his undergraduate studies at the University included a good many of the medical subjects. However, he became interested in investigative work during his undergraduate years, and abandoned medicine for teaching and research. He was greatly influenced in his undergraduate years at the University by his association with Dr. J. F. Snell, who had earlier worked with Wilbur O. Atwater, famous for his untiring studies in the composition of foods, their digestibility and calorie values, and for his studies on the energy requirements of human subjects at rest and doing various kinds of work. Dr. Snell was, therefore, familiar with all that was known about various aspects of biochemistry, including nutrition. Dr. Snell gave Stanley an insight into the methods and technics of research. It may well have been owing to this association that Stanley went to Yale University for post-graduate study in physiological chemistry. At Yale was the laboratory which had been made famous by Russell H. Chittenden, the first well-trained physiological chemist in the United States. Chittenden had, a few years before Stanley entered Yale, become Director of the Sheffield Scientific School, and his distinguished pupil, Lafayette B. Mendel, had taken over the supervision of graduate students and much of the teaching, but Chittenden still continued to give each year a course in nutrition and one in toxicology. So all the graduate students came intimately into contact with both of these superior men. In 1906, Stanley received his

B.A. degree at the University of Cincinnati and went to Yale, where he received his Ph.D. under Mendel in 1908.

The atmosphere of the laboratory of physiological chemistry in those years was highly stimulating. Mendel was at the height of his powers, intensely enthusiastic, studious and a great teacher. No other physiological chemist had so great an influence upon the medical profession as did he, until after Folin and Benedict had made places for themselves as outstanding contributors to the chemistry of physiological processes. Mendel was a great success as a lecturer, and he kept his graduate students occupied with reading in the fields of physiology and biochemistry, so that most of the more important researches of the past and present were critically examined.

The writer became well acquainted with Stanley during the school year 1906-'07, while working in Mendel's laboratory. We sometimes took long walks together in the environs of New Haven and discussed many topics in biochemistry and the future opportunities for research. Stanley was highly enthusiastic about his laboratory work and his reading in scientific literature. He was fast becoming a highly educated specialist. He was to me a congenial companion with a fine sense of humor, and given to poking a little fun at any one with whom his academic activities brought him in contact, who either kept an untidy desk or who was unskillful in manipulations of chemicals or apparatus. His frequent expressions of skepticism concerning statements of men or books on topics which we discussed showed that he was never superficial in his thinking, and never awed by authority. In all our conversations it was evident that he was a thinker and a critic with uncommon natural endowments.

Under Mendel's direction he studied the paths of excretion of several inorganic elements during his post-graduate years but his originality was apparent even at this time. He independently described a new procedure for separation of barium, strontium and calcium, and a new method for distinguishing between glucose and lactose before completing his work for his degree.

The year 1906 marked a turning point in the history of bio-

chemical studies. Otto Folin published in Volume 13 of the *American Journal of Physiology* three papers which immediately brought him to distinction. The first of these described a new system for the analysis of urine for urea, ammonia, creatine, creatinine, and uric acid. Methods hitherto available for quantitative estimation of these substances were either seriously unspecific, as in the case of urea, or required relatively large samples for analysis, as was the case for uric acid. His new procedures were regarded by biochemists and physiologists as so great a step in advance that Harvard University created a professorship in biochemistry for the humble chemist working in the laboratory of the McLean Hospital for Mental Diseases at Waverley, Massachusetts.

Immediately, in laboratories here and abroad, analyses of urines were made from patients with various kinds of disorders, and the more fertile-minded began to set up experiments on animals for the production of pathological states, and to study the composition of the urine by Folin's methods. Folin, himself, did little further in the way of application of his methods to the study of urines. Instead, he turned his attention to refining and improving analytical methods to a degree which would make possible the determination of the constituents of urine in small samples of blood. He was the first, apparently, to realize that it is much more important to know what the kidneys have failed to excrete, among the products of metabolic activity of the body, and which products, accordingly, accumulate to harmful concentrations in the blood, than it is to know what and how much of these have cleared the kidneys. So he reaped a second great triumph when he first made public the technics by which such analyses could be accomplished. Under his leadership investigators turned at once to the applications of his small-sample, high-accuracy methods to the study of normal and pathological problems in physiology. Folin introduced the colorimeter into biochemistry when he employed this instrument for the quantitative estimation of creatine and creatinine, using the color reaction for creatinine described by Jaffe in 1895. This test depended upon the formation of a red color when a

solution of creatinine is treated with picric acid and sodium hydroxide. It reveals the presence of creatinine in a dilution of 1:200,000.

With the publication of Folin's analytical methods for determining quantitatively the principal constituents of urine, clinicians had for the first time methods applicable to the study of patients on a day-to-day analysis of the blood and urine, and the study of disorders of metabolism by the new technics spread rapidly.

For many years the question of the extent of protein digestion in the alimentary tract, the nature of the products absorbed, and the manner in which they were disposed of in the body after being absorbed, had been debated, but no data existed on which a decision between conflicting views on any of these problems could be based. Folin and his students were able to secure data on the concentration of urea, and ammonia in deproteinated samples of blood from branches of the mesenteric veins, the portal vein, the liver, systemic blood and muscle and organ extracts, the samples being taken simultaneously. The non-protein, non-urea, non-ammonia N was correctly judged to consist largely of amino acids. These data afforded an explanation of the course of events in protein digestion and the distribution of amino acids to the tissues during and after the absorption period.

Matters stood thus when Benedict entered upon his vocation of research and teaching. His first appointment was at Syracuse University, where he remained but one year. He then took charge of physiological chemistry at Cornell University College of Medicine, in New York City. Graham Lusk, the distinguished physiologist, was responsible for Benedict's appointment. Benedict often spoke in terms of esteem of Dr. Lusk.

Benedict now entered with enthusiasm upon his career as an improver of analytical methods. Reference to the list of titles of his publications reveals better than can any brief description the nature of the problems which occupied his atten-

tion, and only a few comments on his work will be necessary here.

Benedict found on critical study of the method of Folin for the estimation of urea in urine that along with the decomposition of urea into ammonia and carbon dioxide, some creatinine and uric acid were likewise decomposed to yield the same products. This fact, of course, meant that the values for urea by Folin's method were too high. Benedict proceeded to develop methods for changing urea into its hydrolytic products by gentler means and so arrived at a procedure which gave more nearly accurate values for urea, even when minute samples were analyzed.

Every method which Folin described during the following years was immediately submitted to a critical study and was modified and improved in some important detail by Benedict. Methods for uric acid, creatine and creatinine, total sulfur, sugar, etc., which were devised by Folin, and which at the time of their publication were the best ones known, were, within a few months, tested by Benedict and improved in various ways. It was inevitable that the regular appearance of these follow-up critical studies and replacement of methods devised with great astuteness by Folin, should cause him some irritation. Yet Benedict's great contributions to analytical biochemistry did not in the least detract from the high eminence which was accorded to Folin by chemists, physiologists and clinicians. Folin showed his broadmindedness and tolerance through all the years of Benedict's criticism and replacement of Folin's methods by Benedict's modifications of these methods by always remaining on good terms with him. Indeed, one summer Folin placed his private laboratory at the disposal of Benedict for work which he knew was directed toward improvement on one of his analytical methods.

Especially noteworthy were Benedict's researches on methods for the determination of glucose in blood and urine. Accurate determinations necessitate the finding of chemicals which are reduced by glucose but not by any other substances in blood. Other reducing substances exist in blood and urines,

so the older methods for sugar estimation gave results which were too high. Benedict was untiring through the years of his active life in working toward this objective, and he appears to have achieved the limits of accuracy in this important analysis which has such great significance in the study of normal and perverted carbohydrate metabolism.

Neither Folin nor Benedict was devoted to the study of analytical methods as the primary objective. Both had in mind the application of such methods to the study of normal and pathological body chemistry. This fact is emphasized in the case of Benedict whose first appointment at Cornell University Medical College was in clinical pathology. It was two years later (1912) that he was appointed professor of physiological chemistry, a position which he held with distinction until his death. For many years he supervised the research work on cancer at Memorial Hospital, in New York.

The data which were obtained by application of his methods for sugar, creatine, creatinine, purines, uric acid, sulfur, glutathione and ergothioneine, were the subject of much reflection by Benedict with a view to discerning meaning and interpretations which advanced knowledge. One triumph in this field was his discovery in blood of a sulfur compound hitherto unsuspected as a blood constituent, and which he named *thiasine*. This he later identified as ergothioneine, hitherto known only as a constituent of ergot.

Another discovery of great interest and importance was the existence in beef blood of a compound of uric acid with blood protein. From this compound uric acid is liberated by boiling with hydrochloric acid, but not without some destruction of uric acid. He found that when uric acid was determined in blood with and without the usual precipitation of protein by ammonia-silver-magnesia reagent, the results when the protein precipitation was omitted, were increased by as much as 800 per cent over those obtained in the standard procedure. In chicken blood samples, the increase due to combined uric acid was only about 20 per cent, when the combined uric acid was determined along with the free form. Benedict isolated from ox blood, after

acid hydrolysis of the protein-uric acid complex, as much as 6.7 mg. uric acid, when the colorimetric method showed a total content of 7.0 per cent. This combined uric acid was all in the red cells.

His studies of uric acid metabolism brought to light a biochemical discovery of great interest to geneticists as well as to physiologists and biochemists. Long experience with dogs as experimental animals for the study of metabolic problems had led to the belief that this species dealt with uric acid in a manner different from man and monkey. The latter excrete most of their purine end-products of metabolism derived from degradation of nucleic acids ingested with the food, and those originating from intermediary metabolism, in the form of uric acid. The dog excretes, instead, allantoin. Allantoin is derived from uric acid by the opening of one of its rings, hydrolytically. This process requires the intervention of a hydrolytic enzyme of highly specific nature. In the course of his work with dogs, whose urines were analyzed for uric acid, Benedict observed certain individuals which differed from ordinary dogs in that they excreted uric acid instead of the usual end-product allantoin. Further inquiry showed that the dogs which differed from the usual pattern of purine metabolism and followed the human pattern, were the Dalmatian (Spotted Coach dog) breed. This breed lacks the enzyme which converts uric acid to allantoin, so this substance is excreted without alteration.

Benedict designed and carried through a number of experimental studies on the influence of specific metabolic disturbances on the behavior of malignant tumors. He was one of the earliest investigators to attempt to disturb the metabolic processes of tumors in order to cause regression. He studied the influence of induced diabetes (phlorhizin) on implanted "Buffalo rat sarcoma" on rats. The animals were placed on a carbohydrate-free diet and were at the same time rendered diabetic through the injection of phlorhizin. This treatment was followed by regression of the implants provided they were still small when the treatment was begun. He (with Lewis) found that the changes in the composition of the urines of rats under this

treatment, were duplicated in all respects in a human subject made diabetic by injection of this drug.

He tried (with A. H. Rahe) the influence of diets such as would supply but limited amounts of several of the then known micronutrients (vitamins) on the growth and regression of several types of implanted tumors. This field has since been much more thoroughly explored in the light of more extended knowledge of the astonishing number of essential nutrients not known to exist in 1917, when these studies were made.

Among the most interesting results obtained by Benedict and his associates (especially K. Sugiura) in their studies on malignancy, was the observation that when Rous chicken sarcoma was fractionated by means of half-saturated ammonium sulfate, the tumor-producing substance was carried down by the globulin fraction, apparently none being left in the albumin fraction.

Other efforts to reveal important facts concerning the control of tumor growth, involved studies on the effect of certain dyestuffs on the growth and transplantability of tumors; the influence of high-fat diets; deprivation of animals of vitamin A; the effects of anemia-producing diets; the influence of adrenaline administration, etc. on the ability of animals to sustain the growth of tumor implants. At the time his experiments were conducted in this field the ideas involved in planning the studies were new and novel.

Benedict's services as editor:

Notwithstanding the regular teaching of physiological chemistry to medical students, and his constant occupation with post-graduate students and assistants and associates in the planning and supervision of his many researches, Benedict gave a great amount of time to editorial work. Beginning in 1912, he supervised the Biological Chemistry Section of *Chemical Abstracts* throughout the remainder of his life. In 1925 he accepted the editorship of the *Journal of Biological Chemistry*, a labor to which he gave much time and effort, which ended only with his death.

Miss Mary F. Smalley, who was associated with Benedict

throughout the entire period of his service as Editor of the *Journal of Biological Chemistry*, said in a letter to the writer soon after Benedict's death: "The same characteristics that he showed in his scientific work and in handling the manuscripts submitted to the *Journal* were evident in any phase of the management and publication of the *Journal* that he undertook; a quick understanding of any question; a direct and concise reply, so carefully worded that there could be little chance of misunderstanding; his great feeling of responsibility for the *Journal* in its business administration as well as for the papers published in it.

"Any suggestion or plan that affected the *Journal* he was willing to consider; he was also ready to reconsider, and discard, any of his own directions or suggestions that did not prove feasible. This attitude, of course, added interest and stimulation to the work of this office. Even when he was ill he continued to take care of as much business as he could in addition to the consideration of manuscripts."

Stanley Benedict as others saw him:

Many chemists knew little of Benedict other than through reading year by year his papers in which the shortcomings of a long list of analytical procedures, especially those devised by Folin, were described, and improvements on these procedures were made available. Folin's name was most often recurring in these criticisms only because he was the foremost man of his generation in breaking new ground in devising analytical technics. The general impression gained by many of Benedict's readers was that he was rather unfriendly to Folin. Folin, himself, did not think so, nor did any of us who knew him intimately.

In earlier pages the writer has attempted to give a clear picture of Stanley R. Benedict as a scientist, and of the high standards of his contributions. It is believed that Stanley would value even more than he valued his scientific reputation, the reputation which he had in his personal relations. Soon after Benedict's death the present writer sent letters to a number of persons who had had close contact with him as associates or

students or in the editorial work which is such a fine monument to his industry and interest in the development of biochemistry. The following excerpts are from replies of a German immigrant who served as analyst in Benedict's laboratory for many years, from foreign students, and from his American students. One learns from these excerpts that he was a kind, patient man, who took pleasure in helping others. He was capable of intimate and lasting friendship, and especially among the humble workers about him he had most devoted friends.

Miss Mary F. Smalley, from whose letter I have already quoted, wrote: "While Dr. Benedict could not approach many people easily, I know of many evidences of his deep interest in the welfare of all connected with him. . . . It is not easy to write about Dr. Benedict, for he did not let many know him well. Yet one cannot have been associated with him as long as I was and constantly have seen his methods in dealing with manuscripts and other matters without a certain knowledge of the man and a great respect for his aims and the courage required to carry them out. He was far from indifferent to the criticism he provoked, but accepted this as inevitable."

Emil Osterberg was a member of the staff and associated with Benedict in a number of his researches during a period of twenty-five years. Mr. Osterberg wrote: "The first time I met Dr. Benedict I said to myself, here is where I will have to watch my step. He looked to be a hard man to please. However, it didn't take long for me to find out he was the finest and most good-hearted man I have ever been associated with in my forty-three years experience in laboratories. It was a pleasure to work with him. Once I saw him very happy: when I found uric acid in the Dalmatian dog's urine. The experiments with that dog pleased him immensely.

"During the war in 1917, we tried different methods for making Dakin's solution. One day there was a leak in the chlorine tank, and Dr. B. became more ill than I did. So on the way to the train he got very sick and went to the hospital. I believe he was never in good health after that accident. One

of the colored porters was taken sick. He had a paralytic stroke and was unable to work any more, and was left helpless, with a large family and no income for their support. Dr. Benedict came to the rescue and helped them along. He was always willing to help, and give good advice to everybody. His memory I will cherish as long I as I live."

Olive Hoffman wrote: "Professor Benedict's was an august presence behind which was hidden a shy and sensitive one. This last would not be touched until the first was well worn. As a student of his, the quality which I most appreciated was his patience—one for the lack of which he has often been criticised; not patience of expression, nor with the mechanics of the laboratory, nor with the individual's technique, but in waiting for results. He could wait a long time and not chafe. There would be long silences and no outward encouragement but, if he thought the project worth while, he simply waited. The student might muddle along until he solved the problem, or in desperation sought the Professor. If the latter happened the critical ax usually did not fall. If the former there was a 50:50 chance. At any rate if the point was adequately defended nothing was permitted to interfere with the project, and there was that protection, political and financial, for which a laboratory worker is eternally grateful.

"Once his reserve had been broken, the consistency with which he regarded that individual's ensuing work and opinions sometimes became a fault. To some this made for biased judgment. To others it was the signal for proving him wrong. The latter attitude gave him pleasure, perhaps rather more if he were right. . . . Independence of thought and action was not frowned upon by Professor Benedict. . . . In the laboratory he was a strict disciplinarian. Less than the usual rest periods were taken (no rule against them). . . . His austerity pervaded the laboratory but I am not sure it was invited. There was no illusion that the function of the laboratory was other than work.

"Much of his graduate teaching was done by analysis of past progress, discussion of which was made in the seminar.

At first he would tell us personal anecdotes and characteristics of outstanding biochemists. During the course of the seminars, as this or that paper was discussed, he would take us into the other worker's laboratory by more of these personal anecdotes."

Dr. Isaac Neuwirth, who majored in Benedict's department as a post-graduate student, and who worked with him continuously from 1915 to 1936, said:

"To me, Dr. Benedict was BIG in every sense of the word—big as a man, big as a scholar, big as a scientist. . . . Dr. Benedict was a very *human* individual, although to many he may not have appeared as such."

From Dr. William H. Summerson's reply the following is quoted: "His lectures were frequently devoted almost entirely to an exchange of opinion between himself and the members of the class regarding some phase of the subject on which he felt the class should have reasonable opinions and be able to defend them. The picture I am trying to paint is that of a man with a clear and logical mind, wholeheartedly interested in his work and its development by himself and by others, and scrupulously fair in his dealings with his fellow workers. He had a tolerance of other people's weaknesses, but it was a critical tolerance rather than a passive acceptance. He was an inspiration to me, and as far as I could see, to all those students who came into contact with him."

Dr. K. Sugiura, who worked continuously with Benedict from 1917 to 1936, wrote of him: "His charming smile and his gracious manner impressed me greatly. I thought of him as a great teacher, and my future adviser. . . . During my twenty years of association with Dr. Benedict, I never saw him with an angry face. . . . Dr. Benedict was well liked by his students. . . . He was an unselfish man. He gave full credit to his associates in many joint scientific discoveries, and was intensely pleased to do so."

Dr. Joseph C. Block, who was associated with Benedict for many years in research said of him: "While he was always critical his criticism was constructive and tempered with kindness. . . . I never heard him swear. . . . In one of his rare con-

fidential moods he told me that he would prefer teaching philosophy to biochemistry. . . . He loved to tinker but he was not very skillful with his hands. . . . He was rather generous with money, at one time when he found out that I was rather hard up he felt very much hurt because I had not come to him for a loan. . . . He had a very quick temper but if he hurt anybody by his words his regrets were forthcoming immediately. . . . He was very bashful in the presence of ladies. . . . He was very kind to animals. Whenever he had to inflict pain, for instance, like an injection, he would always talk very kindly to the animal during the whole procedure."

His sister, Dr. Mary K. Benedict wrote that he was always fond of cats. She once saw five saucers on the kitchen floor at one time, containing different morsels for his cat, so that it might make its choice of what it liked best. . . . that once a bird built its nest over his front door, and he would not disturb it, so used the kitchen door until the bird had left its nest . . . that he became fond of photography early in life and did some fine amateur work. Dr. Neuwirth also said that one of Stanley's favorite activities, during his mature years, was taking pictures.

Dr. A. Goudsmit, Jr., wrote: "When Mrs. Goudsmit and I came to this country in September, 1933, as Fellow of the Netherland-American Foundation, Dr. Benedict was the first educated American we met. I remember very vividly how he appeared to us that morning behind his desk: his peculiar head, decidedly too small for the size of his body, looking as if it had been the subject of a smallpox attack and as if it was exposed to the influence of a rough climate at all times; his conspicuously erect posture, walking as well as seated; his continuous smoking; his matter-of-fact and direct way of attacking the problem in hand; all these things impressed us at our first encounter of the typical sea captain. And he has many times since done so. . . . Socially, he no doubt was extremely retiring. Because he left the city every night he made it all the more difficult for his associates to have contact with him outside of 'business.' Only a very few favorites of

his department, and I suspect still less outside of it, were ever invited out to his Westchester house."

Dr. Jeanette A. Behre, who was for years associated with Benedict as a student, and later in research, wrote of him:

"I think of Dr. Benedict as a person of unusual vitality. His personality always completely dominated the department. . . . There was something magnetic about his personality and something fundamental about his enthusiasm for scientific work which could not help being inspiring. . . . He disliked most social gatherings although he could be delightful company. . . . The country appealed to him partly because he was very sensitive to noise, but he also had a sincere and fundamental love of nature. . . . He had many inner resources and interests. He had studied philosophy intensively at one time and philosophical speculation always had a particular fascination for him. He read literature and poetry. . . . Conrad was one of his favorite books, and he liked Poe's poetry, to mention only a few. He always had definite and original reactions to whatever he read. He was also fond of detective stories. He did a great deal of photography, and in summer especially, he enjoyed his motor boats. I cannot hope to describe his sense of humor, which was an intrinsic part of his personality and which could be both witty and whimsical. None of his friends can ever forget its particular quality nor the expression on his face when something amused him. He had a quick temper, which he often regretted, and which many people found hard to understand. There are plenty of us who have been hurt at times by his frank criticisms. No one who really knew him could fail to appreciate his directness and sincerity or to realize his fundamental kindness. There are many people who could testify to his consideration and generosity, to his genuine sympathy for personal troubles which were brought to him and to the great amount of help and encouragement which he gave not only to professional associates but also to many other people with whom he came in contact. He had a standard of personal integrity from which nothing could make him deviate and he was never afraid to fight for what he considered just

or right. Perhaps relatively few people realized that in spite of his assurance he was very sensitive to criticism and deeply modest about himself and his own accomplishments."

Stanley Benedict's wife, Ruth Fulton Benedict, a distinguished anthropologist, whom he married in 1914, said that in his adult life he never had chemicals nor chemical books at home. All the time outside the office and laboratory was devoted to recuperating his energies and in these relaxation periods he turned to hobbies which would take his mind off his work. Two of these hobbies were special favorites—engines which he set up and took to pieces, whether they were in pumps, boats or automobiles; and photography, where he especially enjoyed playing with different chemical processes in developing and printing.

His summers were devoted to recuperation as far as his duties as editor of the *Journal* would allow. He loved the isolation and leisure of a New Hampshire summer; for many years he spent the entire summer vacation period on the shores of Lake Winnepesaukee. Still more he enjoyed traveling, and went to the Canadian Rockies or to Mt. Ranier Park or took the boat trip to Alaska or to the North Cape. He struggled always, under the difficulties of an unusually high blood pressure, and solved these difficulties as far as he could by leading a regular life with as few intrusions as possible.

Stanley Benedict's qualities may be summarized by saying that he was a man of superior mental endowment, who delighted in constructive thought; whose interests aroused activity in experimental inquiry. He was strongly motivated by a sense of duty, and served well his fellow scientists and mankind in general by his researches and editorial work.

As a teacher and director of research and as personal counsellor he sustained high standards, and in return he won from those associated with him, respect, admiration, friendship, loyalty and gratitude.

Honors and memberships in scientific societies:

Member of the National Academy of Sciences

American Association for the Advancement of Science

American Society of Biological Chemists (President 1919-1920)

American Physiological Society

Phi Beta Kappa

Corresponding Member of the Société Biologie de Paris

Sigma Xi

Alpha Omega Alpha

The Harvey Society

Associate Fellow of the New York Academy of Medicine

KEY TO ABBREVIATIONS USED IN BIBLIOGRAPHY

- Am. J. Cancer = American Journal of Cancer
 Am. J. Physiol. = American Journal of Physiology
 Arch. Int. Med. = Archives of Internal Medicine
 Biochem. Bull. = Biochemical Bulletin
 Biochem. J. = Biochemical Journal
 J. Am. Chem. Soc. = Journal of the American Chemical Society
 J. Biol. Chem. = Journal of Biological Chemistry
 J. Cancer Res. = Journal of Cancer Research
 J. Lab. Clin. Med. = Journal of Laboratory and Clinical Medicine
 N. Y. Med. J. = New York Medical Journal
 Proc. Am. Soc. Biol. Chem. = Proceedings, American Society of Biological Chemists
 Proc. Soc. Exp. Biol. Med. = Proceedings, Society for Experimental Biology and Medicine
 Surg. Gynec. Obst. = Surgery, Gynecology and Obstetrics
 Zeit. physiol. Chem. = Zeitschrift für physiologische Chemie

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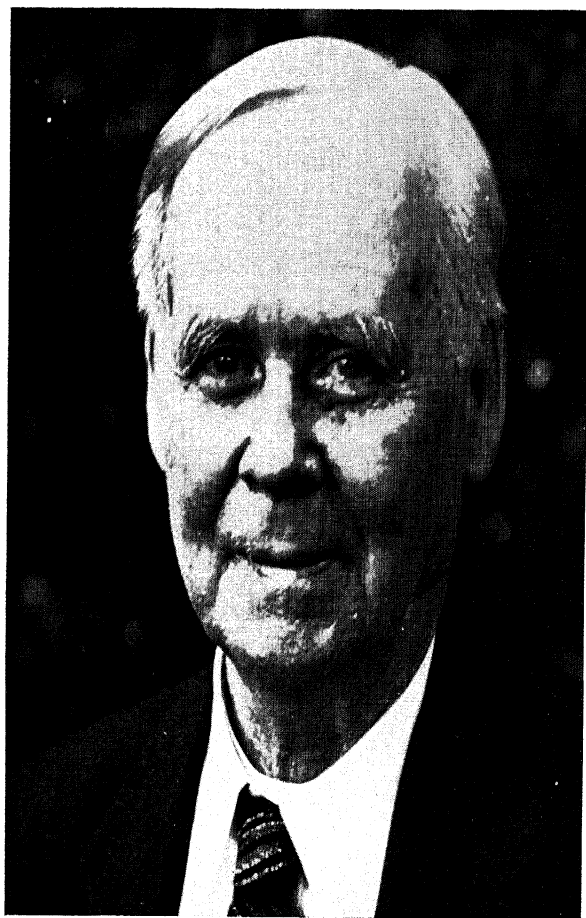
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William Albert Noyes

WILLIAM ALBERT NOYES¹

1857-1941

BY ROGER ADAMS

William Albert Noyes was born November 6, 1857 on a farm near Independence, Iowa. He was the seventh generation of the Noyes family in America, being descended from Nicholas Noyes who came with his elder brother, James, from Choulderton, Wiltshire, in 1633. Their father had been a clergyman but had died before the sons left England. The two brothers settled first in Medford, Massachusetts but in 1655 moved to Newbury. Samuel, of the third generation in this direct line, moved to Abington about 1712 and there he and the succeeding generations lived for over one hundred and forty years.

Spencer W. Noyes, the great grandson of Samuel, was admitted as a student to Phillips Academy at Andover, Massachusetts, March 21, 1837. He married Mary Packard—related to the famous family of shoe manufacturers—and they had three children when they left Abington and migrated to Iowa in 1855. When the father with his family went west to open up a homestead, he found conditions typical of the prairies and life was necessarily primitive. In the east he had been a cobbler by trade so had had little experience working the land. The early years were difficult ones because of the hard work in establishing a farm home, getting land into cultivation, raising food and storing sufficient for winter needs. This had to be accomplished by much manual labor with only the simplest tools and equipment. Frontier living had the usual monotony of daily routine, broken by Sunday observances, by the only big family festival at Christmas, and by the not infrequent excitement when prairie fires threatened.

The family life was an especially happy one even with the strict financial limitations, for there was the wholehearted cooperation of all; the girls helping the mother with household tasks and the boys learning by working with the father. It was

¹ Grateful acknowledgment is made to the sons of Dr. W. A. Noyes for help with historical material and to his wife, Katharine Macy Noyes, for reading the manuscript before publication.

necessary to fashion and repair many articles for farm and home use, and William Albert became skillful with his hands. Not only tools, but the toys of the home were often home-made, for playthings purchased at distant stores were few. In later years William Albert Noyes recalled as one of the vivid incidents of his youth his longing for a sled at Christmas, but which didn't arrive until months later, due probably to his father's limited funds at the holiday time. He also remembered spending a whole season digging potatoes to earn enough money for a pair of skates. He was an active youth and enjoyed the outdoor sports of the normal boy.

The intellectual life in the home was also one of cooperation. Mother Noyes was a cultured woman, never really at home on the prairie. She was the driving force in the family and created the intellectual atmosphere which inspired all four children to go to college and enter professional life. She taught the older children and they, always having shown great interest in the younger brother, helped in the instruction of young William; the boys with farm practices and elder sister Hannah with his books, especially Latin. He, from the beginning able to concentrate, learned the Greek alphabet one day while plowing corn in the fields. Although he walked miles to and from the country school a few months each year, he was self-taught to a great degree. All New England families on migrating to the west carried among their precious possessions the Bible and a few books. In the scanty library of the father was a copy of "Elements of Chemistry" by J. L. Comstock, M.D., published in 1831 at Hartford, Connecticut. Young Noyes acquired an early interest in science by reading this and began experimenting for himself with materials purchased in the village apothecary's store.

To fully appreciate the cultural influences of the frontier home it is of interest to mention the careers of the three older children. Edmund, the oldest son, was too young to be in the Civil War. After studying as a dentist's apprentice in Dubuque, he went to Chicago prior to 1870 and practiced dentistry in an office not far from the present Marshall Field Building. Burned out in the Chicago fire, he revived his practice and accepted

along with it an appointment as Professor at Northwestern University which he held for nearly fifty years.

The older girl, Hannah, later fondly known by the boys in the W. A. Noyes family as "Aunt Nannie" was graduated from Iowa College in 1875 and had a long and distinguished career as a teacher, author and lecturer. She married one of her college classmates, Charles Davidson, who subsequently received the Doctor of Philosophy degree in English at Yale University. Hannah Noyes Davidson's best known works were the editing of the Autobiography of Benjamin Franklin and her Study Guides on Literary Works. The other sister, Mary, was one of the first women to receive the degree of Doctor of Philosophy in Physics from an American university. This was conferred by the State University of Iowa in 1892. Her teaching career at Oxford College for Women was terminated by ill health in the early 1900's.

The real turning point in Noyes' life came when he went to Grinnell, Iowa, and enrolled for one semester in the Academy before entering Iowa College in 1875. This college, founded in 1847 by three pioneer settlers, Congregationalists and graduates of Yale, and twelve graduates of Andover Theological Seminary, became in 1909 Grinnell College. Here was provided a religious background, a follow-up of the home training which influenced Noyes throughout his lifetime. It was also at Grinnell that he had his thorough study in the classics and awoke to a real interest in science. At that time Grinnell had a three-term system, fall, winter and spring. It was necessary for Noyes to absent himself from the winter terms in order to earn money for his expenses for the other two sessions by teaching in nearby country schools. This did not deter him from graduating in the normal period of time, for by night study he covered the subjects missed in the regular classes of the winter terms. During his senior year the financial stringency was somewhat relieved, as he was honored by receiving one of the four coveted Grimes Foundation Scholarships "awarded to the best scholars and the most promising in any department . . . without regard to religious tenets or opinions entertained by any person seeking said scholarships." Perhaps

this absence of bigotry in a denominational college was reflected in Noyes' characteristics of tolerance and love of justice. During the college course he was registered as a "classical student," and specialized particularly in Greek. He also had political science under the distinguished Professor Jesse Macy. No doubt these subjects formed the foundation of his interest later in life in philosophical discussions and international issues. As an extra-curricular activity he was an enthusiastic member of the Chrestomathia Literary Society. This classical training and ability to express his thoughts in words gave to his work and writing all through his life profound depth and clarity.

While studying at Grinnell, he became interested in natural philosophy. He took such scientific courses as were available in those days to a classical student, but these were confined chiefly to his junior year. However, driven by a consuming interest, he worked regularly at night in the chemical laboratory for recreation while other students sought diversion in other ways. He thus completed the study of qualitative and quantitative analysis by himself. When time came for graduation, Noyes pointed out to the faculty that he had not only finished all of the work required for the Bachelor of Arts degree, but he had also met the requirements for the Bachelor of Science degree, and he claimed the right to have both degrees awarded simultaneously. After some debate, the faculty was forced to admit his contention and he was granted the A.B. and B.S. degrees in 1879. The faculty passed a rule, however, which prevented any simultaneous awarding of two degrees thereafter.

During the year following graduation he taught Greek and chemistry thirty hours a week in the Academy of the College, and during his spare time did advanced work in analytical chemistry. He studied blowpipe analysis and built himself a small assay furnace so that he was able to eke out a small income by performing analyses. In the fall of 1880, the second year after graduation, he was given charge of the Department of Chemistry for one term while Professor W. H. Herrick was absent on leave. In 1880 he was awarded the Master of Arts degree by Grinnell College.

With what savings he had he went to Johns Hopkins Univer-

sity during the academic year 1880-1881, but did not arrive in time for the opening of the fall term. While there he worked under Professor Ira Remsen, and performed water analyses on the side to pay his expenses. His life at Johns Hopkins was one mainly of work for he received the Doctor of Philosophy degree in 1882 after one year and a half. When Dean K. C. Babcock of the University of Illinois later asked Dr. Remsen whether he was not somewhat regretful at letting a student finish work for the degree in such a short time, Remsen merely asked, "Has anyone ever objected?" This was indicative of the high esteem which he had for Noyes. During the summer of 1881, Noyes made four-hundred water analyses for Professor G. W. Mallet of the University of Virginia, who was studying the connection between water supply and disease for the then-existent National Board of Health.

The first independent position Dr. Noyes held was that of instructor in chemistry at the University of Minnesota during 1882-1883 at a salary of \$700. Much of his time that year he spent in analytical work for the Minnesota Geological Survey. In the fall of 1883 he became Professor of Chemistry at the University of Tennessee at Knoxville. While Noyes was at the University of Tennessee, professorial appointments had to be approved annually by the State Legislature. The other professor in the Department was a southerner, and there still existed considerable feeling because of the Civil War so that Noyes considered his position to be insecure. Consequently in 1886 he accepted a professorship at the Rose Polytechnic Institute in Terre Haute, Indiana.

No chemist worthy of the name in the United States during that period considered his training complete until he had spent some time in Europe. Consequently, Dr. Noyes went on leave during the second semester of 1888-1889 and worked with Professor Adolf von Baeyer, the distinguished organic chemist at the University of Munich. The months with von Baeyer exerted a profound influence on him and the subsequent travels around Europe, particularly in England, widened his acquaintances and cemented friendships which lasted throughout his life. Alexander Smith was a student at Munich at the same

time and, on returning home, Noyes was instrumental in getting him his first position in the United States at Wabash College. J. Bishop Tingle, later a professor at McMaster University, Hamilton, Ontario, was another intimate friend.

At Rose Polytechnic Institute, Professor Noyes' salary was \$1850 per year, later raised to \$2250, and his starting salary was the highest paid to any professor of chemistry west of the Alleghenies at that time. Nevertheless, it proved insufficient to support a family, and he always did some work for the State Water Survey and a certain amount of analytical work. Professor Noyes *was* the Chemistry Department at Rose Polytechnic Institute, and for many years he taught all of the courses of the chemistry curriculum. The seniors in the curriculum were required to present theses based on original research. Several of these were published in the *American Chemical Journal*, and Noyes' early scientific reputation was based on this work. Some students from those days, particularly George W. Morey, later attained eminence.

In 1901 Professor Noyes became Editor of the *Journal of the American Chemical Society*. In 1907 he founded *Chemical Abstracts*. It was an enormous undertaking to get this latter project under way since he even had to correct proof because of lack of assistants. But two years later when he was relieved of this additional burden, *Chemical Abstracts* was on a sound basis and has become one of the outstanding publications in science. From 1904 to 1907 he was also secretary of the American Chemical Society. In 1909 he was a member of the committee which recommended publication of the *Journal of Industrial and Engineering Chemistry*. Later Professor Noyes took an active part in the establishment of *Chemical Monographs* (1919) and *Chemical Reviews* (1924). He contributed more than any other individual to the publications of the American Chemical Society and largely to his influence may be attributed the healthy start and growth of these publications which today hold a preeminent position among the scientific journals of the world.

It had become evident to Professor Noyes by 1902 that the Rose Polytechnic Institute would be surpassed by many other

institutions in that area, and when he was asked to become the first Chief Chemist of the newly established Bureau of Standards, he took the necessary Civil Service examinations and obtained the appointment. He left Terre Haute during the summer of 1903, but since the new buildings for the National Bureau of Standards were not complete, he returned to Johns Hopkins University to do research during the academic year 1903-1904. This location was chosen because of the proximity of Baltimore to Washington which permitted him to participate in the plans and construction of the new buildings.

It was during the period in Washington from 1904 to 1907 that Noyes' reputation became truly international. He was relieved of teaching duties and his researches prospered. His development of standard methods of analysis and standard specifications for chemicals established the base upon which the Bureau of Standards expanded to its present position of influence and prestige. Famous foreign and American visitors frequented his home during that period: Ostwald, van't Hoff, Harvey Wiley, W. F. Hillebrand, not to mention many other chemists.

After the death of Professor A. W. Palmer, Head of the Chemistry Department at the University of Illinois, a search was made for a new Department Chairman and the position was offered to Dr. Noyes. He declined, but the following year when the University, intent on securing him, made a new offer he accepted. President Edmund J. James assigned to him the task of "building a strong graduate Department of Chemistry." For the next nineteen years, Dr. Noyes' major effort was to strengthen the department by expanding the staff with promising young men, reorganizing the courses of study, and systematizing the plan of instruction. By his own interest in research he stimulated his colleagues in original work and with staff cooperation he laid the foundation for one of the eminent chemistry departments of the country. The reputation of Illinois chemistry, the vigorous atmosphere for progress which he inspired, and his traditionally friendly attitude toward the growing staff will always remain as his lasting contribution to the University of Illinois and American chemistry.

In 1907 the entire teaching staff of the department numbered eleven and there were seventeen graduate students. At the time of his retirement in 1926, the staff had increased to twenty-five and the graduate students to one hundred fourteen. But of more importance may be cited the scholarly work achieved. During the period 1870-1903, there was an average of one paper a year from the department; from 1904-1906, an average of fifteen papers annually; during the period Dr. Noyes was director of the laboratories, 1907-1926, the number of publications averaged forty-four a year. His personality and great interest in research will always be remembered by the members of his staff.

His breadth of knowledge in various fields of chemistry profoundly impressed his younger colleagues whose training had been far more specialized. He initiated weekly journal meetings, monthly faculty luncheons and the Chemistry Club which served to build up friendship and cooperation among students and faculty.

Dr. Noyes received many honors and deserved recognition for his achievements. He was awarded the honorary degrees of LL.D. from Clark University in 1909, Chem. D. from the University of Pittsburgh in 1920 and Sc. D. from Grinnell in 1929, the fiftieth anniversary of his graduation. He was elected in 1908 an Honorary Member of the Philadelphia College of Pharmacy. He was a fellow of the American Academy of Arts and Sciences, a member of the National Academy of Sciences and the American Philosophical Society. He served the American Chemical Society as Editor, Secretary, and in 1920 as President. He was Secretary of Section C of the American Association for the Advancement of Science and in 1896 Vice-President. He was a charter member of the Indiana Academy of Science and its President in 1912. He was also a member of the Illinois Academy of Science, the Society of Chemical Industry, the Deutsche Chemische Gesellschaft and the Société Chimie Industrielle. Dr. Noyes served on the Illinois State Board of Natural Resources and Conservation as member and Secretary from 1917 until his death.

In 1908 Dr. Noyes and Dr. H. C. P. Weber were awarded

the Nichols Medal for their work on the atomic weight of chlorine. He received the Willard Gibbs Medal in 1920 for his distinguished attainments in organic chemistry and the Priestley Medal, the highest award of the American Chemical Society, in 1935, for his general contributions to chemists and chemistry.

On November 5, 1937 the staff of the Chemistry Department at the University of Illinois united in celebrating Professor Noyes' eightieth birthday and presented to him a parchment scroll signed by his colleagues of the teaching staff. The inscription reads:

"The celebration of your eightieth birthday is an event which we, your colleagues, are happy and proud to share. We realize that our privilege on this occasion is one which belongs not to us alone, but rather to the entire chemical world. Chemists everywhere—of every persuasion and of every nation—share the feeling of gratitude and high esteem which we would express.

"Your numerous scientific discoveries, your successful promotion of chemical societies and journals, and your inspiring guidance of students and colleagues have made your name immortal among men of science. We take this opportunity to congratulate you upon your achievements and to wish you continued success and happiness."

On May 13, 1939 at a University Convocation at the University of Illinois, the Chemical Laboratory was rededicated and named "The William Albert Noyes Laboratory of Chemistry". Dean M. T. McClure on this occasion remarked "Professor Noyes, through his long and distinguished connection with this Laboratory, has brought honor to the University of Illinois. In return the University seeks to honor Professor Noyes. This is no casual recognition. Only one other building on the campus has been named in honor of one of its living scholars. So long as the University stands, the name of Professor Noyes will remain among its immortals, among those who have devoted themselves to the mystery of the mechanism of Nature. Any sound philosophy must look to science for its facts and to the lives of men for its ideals."

While Professor Noyes was at Knoxville, Tennessee, in 1883 he married Flora Collier, a former student in the Conservatory

at Grinnell College, whom he had met while an undergraduate there. A daughter, Helen, was born in Knoxville. During the Noyes' trip to Europe in 1889 she contracted spinal meningitis and died. A second daughter who was born in 1892 died from typhoid fever in 1899. A third child, William Albert, Jr., was born in 1898. He has now become one of America's most distinguished chemists. He is head of the Chemistry Department at the University of Rochester, Past-President of the American Chemical Society. He was Editor of *Chemical Reviews* for many years, and is now Editor of the *Journal of the American Chemical Society*. He is a member of the National Academy of Sciences and the American Philosophical Society. Flora Collier Noyes died in 1900.

On June 18, 1902 Professor Noyes married Mattie Elwell whom he had met while in Minnesota. In 1904 a son, Charles Edmund Noyes, was born. He followed a literary career and is now Publicity Director of the American Institute of Accountants. The second Mrs. Noyes died in 1914.

In November 1915, he married Katharine Haworth Macy, daughter of Professor Jesse Macy, an internationally known scholar in political science at Grinnell College who had been one of Dr. Noyes' teachers. At the time of her marriage Katharine Macy was teaching English at the institution. They had two sons; Richard Macy Noyes, now Assistant Professor of Chemistry at Columbia University, was born in April, 1919, and Henry Pierre Noyes, now a graduate student in physics at the University of California, was born in Paris in December, 1923.

After the close of World War I Professor Noyes, at the invitation of Professor Ernest Cohen of Utrecht, visited Europe to attend the meeting of Allied and Central Power chemists. The following year, in 1923, he was a delegate to the International Union of Chemistry meeting at Cambridge, England and on this trip spent much time on the Continent trying to allay some of the hatreds between the French and Germans resulting from the war. His last trip to Europe was in 1931 in order to give his two young boys a chance to become familiar with foreign languages.

Before the first World War in the peaceful year of 1910, he built a family cottage near Frankfort, Michigan which was to be his summer home nearly every year from then until he died. As an ardent churchman, a Deacon for many years of his life, it was perhaps natural that this cottage should have been built on the grounds of the Congregational Summer Assembly. While he did considerable writing in the summer, it was in Michigan he indulged in chopping wood, rowing a boat and strolling about the country.

Anecdotes about Professor Noyes and his family life furnish a little more intimate knowledge of him in matters outside of his scientific career.

In 1902 his second wife received as a wedding gift from her brother, who had a real interest in horses, a small pony which was named "Teddy" after the first Roosevelt. This pony was with the family, in Terre Haute, in Baltimore, in Washington and finally in Urbana, until he was put out to pasture by arrangement with a local farmer in 1917 when an automobile finally took his place. *The Daily Illini*, University of Illinois student newspaper, referred to "Little Willie and his coach and four" since the family was often seen driving around Urbana in a little two-seated buggy behind this pony.

Professor Noyes always believed in close family ties and these included the relatives of his three wives. During the last years of his life he wrote a "family letter" every week and carbon copies were distributed widely among the members of the family. Sunday was observed strictly in the New England tradition, Sunday School, church and a formal dinner at one. Reading out loud or being read to were among his greatest enjoyments. This he often did with his wife and children. Novels and sometimes more serious philosophical books were read but seldom anything too light and flippant.

At home his meals were fairly formal affairs in the dining room, for breakfast nooks had not come into existence and his memory went back to eating in the kitchen of the early farm home. Grace was always said and conversation was usually serious and intellectual. The days' activities were recounted with the family and he liked to discuss important matters with

his wife. He did not unbend easily and indulge in banter, but he did appreciate a good joke. Entertaining was infrequent and formal. Once a year there was a formal reception for the department at which evening dress was in order. Dinners when the President or the Dean was invited were given in Victorian style.

Professor Noyes usually worked in his study at home after supper. His duties for the American Chemical Society and the books he wrote occupied many of his evenings. He had a very complete library which included not merely scientific books but those by the best literary authors. Although he gave the impression of never relaxing completely, he was truly absent-minded at times and had powers of concentration which were remarkable. Noise and confusion were not tolerated about the home, and he never permitted a rough house since they interfered with his concentration and proofreading. He was busy all day whether he was in Urbana or at his cottage in Michigan.

Professor Noyes was meticulous in his personal affairs. His accounts were carefully kept, personal correspondence was retained, and his desk at home was covered with papers more than his desk at the laboratory. His habit of doing everything on an "urgent" basis was characteristic of the man and persisted after he retired and until his death. He was too active too long to enjoy fully the years of much deserved leisure.

He took little interest in menial tasks about the home or outdoor work in the yard. Such things seemed to bore him, perhaps as a result of his many years of hard work on a farm while a youngster or because of his need for every minute for intellectual pursuits. Nevertheless, he was ingenious and could make repairs of household appliances and get them in working order. When he found the two older sons trying to learn chess from the *Encyclopaedia Britannica* he revived his early skill and made a set of chessmen for them. His health was excellent, but he rarely took any exercise in Urbana after his five minute setting-up exercises each morning, other than walking to and from the laboratory. It was consequently surprising that he was always in good shape when it came to a hike whether in Glacier Park or in Europe.

Professor Noyes attended many lectures and rarely missed an important lecture or concert given on the University campus. His interests were catholic particularly as regards world affairs and politics. He also thoroughly enjoyed the legitimate theatre and went to everything offered locally in the days when good companies played in Champaign. When in the larger cities either in the United States or abroad he always went to plays. While he did not care much for music himself, he did go to hear outstanding artists and saw that his children had musical opportunities.

In spite of his mild manner of speech, Professor Noyes left no uncertainty as to his meaning and would defend his viewpoint with skill, courage and fearlessness. He was unafraid of an adversary in debate, so long as he believed that truth and right were on his side. This quality of stubbornness served him well, for it made possible the attainment of adequate support for his department and of success in many activities in and out of science for which he had been given responsibility.

He was particularly sympathetic and helpful to his students and colleagues. He had the faculty of overlooking in an individual any idiosyncracies which might prejudice most people, and always gave sound advice. He was profoundly respected by all scientists, and was looked upon with real affection by the younger men who had the good fortune to come under his guidance. He maintained a remarkable degree of interest in persons he had known and worked with. His colleagues will remember his untiring devotion to his tasks, his simplicity of life, his fairness to all, and his devotion to the cause of peace.

During his life, Professor Noyes had more than his share of sorrow in the loss of loved ones, and he always carried a heavy load because of being a pioneer in chemistry and lacking facilities everyone now expects and takes for granted. Through it all he kept the same outward calm. He thought everything out carefully and lived a carefully planned life. He could make decisions rapidly, but he never made them "hastily".

After his retirement in 1926, Noyes' study of chemistry never ceased. He was as alert and excited by all new developments as his younger colleagues and continued his researches until the

time of his death. His attendance at scientific lectures and meetings of the American Chemical Society and National Academy of Sciences was as faithful as in his more active years. Retired from his administrative duties, he had more time for reading and writing. He enlarged his interest in religious, philosophical and political problems. Always deeply religious, he devoted even greater service to the Congregational Church. While still in active service he took an active part in planning the building of the present church edifice which serves both Urbana and Champaign as well as the student body of the University.

He increased his efforts toward the creation of world peace and avoidance of future wars. He had many warm friends in both France and Germany and he was deeply distressed because of the bitterness which had grown up between these countries. He carried on an extensive correspondence with the scientific leaders of many nations and the publications of many of these letters show the increasing determination and earnestness with which he approached the involved international situation. He was convinced that scientists, accustomed to the recognition of truth and the appreciation of honest effort, ought to find a common ground for mutual understanding. He spoke earnestly for peace, disarmament and good will. Some of the topics of his pamphlets indicate his breadth of understanding and his sincere effort to abolish war: "Building for Peace", "International Understanding", "War or Peace", "Laissez Faire or Cooperation", "International Backgrounds", "Who Have Paid the Cost of War?", "Science in Place of War", and many others were written between 1920 and 1937. Dr. Noyes was a charter member of the American Committee for Democracy and Intellectual Freedom and even to the last few days of his life took an active part in the work of this organization. He believed in evolutionary progress almost as an act of faith. He maintained that he had lived to see substantial improvement in relations between people on both the domestic and international level, and was convinced, in spite of setbacks, in the ultimate triumph of cooperation and understanding among peoples. In his great zeal and striving for international peace and his participation in religious activities his wife, Katharine Macy Noyes,

was his devoted coworker and helpmate. Their common interests in the four sons and similarity of tastes in intellectual life made for a companionship of affection and loyalty.

In October 1941 Professor and Mrs. Noyes motored to Madison, Wisconsin to be present at the fall meeting of the National Academy of Sciences. The following week at home he attended the all-day meeting of the Illinois Board of Natural Resources. Just two weeks before his eighty-fourth birthday William Albert Noyes passed away after a brief illness of five days. Funeral services were held in the First Congregational Church in Champaign and, after cremation in Chicago, the ashes were interred in the Macy family lot in Grinnell, Iowa.

The "*Illio of 1942*", the Senior Class Book at the University of Illinois, was dedicated to Dr. Noyes. His successor as Head of the Chemistry Department was asked to write the dedicatory words. The following tribute to him from the *Illio* characterizes briefly the place of this distinguished man on the campus, in the community, and in the professional world.

"William Albert Noyes

1857-1941

"William Albert Noyes, Professor and Head of the Department of Chemistry, came to the University of Illinois in 1907, and for thirty-four years devoted himself to the utmost in the furtherance of its interest.

"Coming at a time when the science of chemistry in this country was entering a new era, he brought to the department a background of culture, a training in investigational science, editorial and executive experience. With his inquiring mind and tireless vigor he laid the foundation of a department in which fundamental courses were nurtured and research became a vital interest.

"Patient, helpful, sincere to student and colleague; tenacious and undiscouraged in attaining a goal; zealous in working for the right in a world so apparently athwart his ideals, he was an influence throughout the life of the whole University.

"To few are given such outstanding achievements of scholarly research, of developing a department whose building now honors

his name, of authorship of many books and scientific papers, of fostering new journals which have become leaders in the fields of chemistry, of giving strength and vision to many chemical organizations from their modest beginnings until they attained national prestige. With all of this he was a lover of home and friends. Of him it can be truly said he was a faithful servant of the Church and the State."

Publications

The scientific achievements of Dr. Noyes were characterized by their diversity and by the care and skill with which they were performed. He completed investigations in the fields of analytical and inorganic chemistry as well as in his specialty, organic chemistry.

His work as a student with Remsen aroused his interest in the variable activity of different oxidizing agents upon alkylated benzenes. As a consequence he pursued this field further and in his first independent series of papers demonstrated the effectiveness of the previously unstudied ferricyanides for converting certain alkyl benzenes to the corresponding benzoic acids.

Dr. Noyes had been engaged in much analytical work in his early days partly through interest and partly by necessity. It is not surprising therefore that he undertook to improve various accepted procedures. His contributions in this field were numerous and included the detection and estimation of titanium, the determination of magnesium, sulphur and phosphorus in iron and steel, a means of estimating the amount of benzene in illuminating gas, and the detection of strychnine in the human body.

The elucidation of the structure of camphor was being explored in many laboratories in different parts of the world during the period 1880-1900. Dr. Noyes was one who became attracted to this problem and whose subsequent researches served as a major contribution toward its final solution. His early work was confined to a study of camphoric acid, the major oxidation product of camphor, and to related compounds. Fifteen papers were published on this subject. He turned his attention next to the molecular rearrangements in the camphor

series and achieved much in the clarification of these complicated reactions. The results appeared in about twenty articles. Dr. Noyes never lost his interest in camphor and he continued some phase of investigation in this field over a period of forty years.

His exploits in inorganic chemistry resulted in 1889-1890 in the determination of the atomic weight of oxygen with more accuracy than had been attained hitherto.

Dr. Noyes was one of the first organic chemists to become interested in the electronic theory of valence, and as early as 1912 gave a presidential address before the Illinois State Academy of Sciences on this subject. That same year he published two articles attempting to explain certain phenomena of ionization by the electron theory and the connection between electronic conductivity and loss of electrons by metals. Each year thereafter one or more papers on the application of the electron theory to organic and inorganic compounds appeared, especially as applied to polarity of valences and in relation to ionization. He was particularly interested in the valence and character of the nitrogen atom in ammonium salts, in nitrogen trichloride and other nitrogen compounds.

KEY TO ABBREVIATIONS

- Am. Chem. J.=American Chemical Journal
 Ber.=Berichte der Deutschen Chemische Gesellschaft
 Bull. soc. chim.=Bulletin, Société de Chimie
 Chem. Revs.=Chemical Reviews
 Ind. Eng. Chem.=Industrial and Engineering Chemistry
 J. Am. Chem. Soc.=Journal of the American Chemical Society
 J. Anal. Appl. Chem.=Journal of Analytical and Applied Chemistry
 J. Chem. Ed.=Journal of Chemical Education
 J. Franklin Inst.=Journal of the Franklin Institute
 Nat. Acad. Sci. Biogr. Mem.=National Academy of Sciences Biographical
 Memoirs
 Proc. Am. Acad. Arts Sci.=Proceedings, American Academy of Arts and
 Sciences
 Proc. Am. Assoc. Adv. Sci.=Proceedings, American Association for the
 Advancement of Science
 Proc. Am. Phil. Soc.=Proceedings, American Philosophical Society
 Proc. Am. Soc. Test. Mat.=Proceedings, American Society for Testing
 Materials
 Proc. Ill. Acad. Sci.=Proceedings, Illinois Academy of Science
 Proc. Ind. Acad. Sci.=Proceedings, Indiana Academy of Science
 Proc. Nat. Acad. Sci.=Proceedings, National Academy of Sciences
 Rec. trav. chim.=Recueil des travaux chimiques des Pays-bas
 Sci. Mo.=Scientific Monthly
 Trans. Faraday Soc.=Transactions, Faraday Society
 Trans. Ill. State Acad. Sci.=Transactions, Illinois State Academy of
 Science
 Zeit. anal. Chem.=Zeitschrift für analytische chemie
 Zeit. angew. Chem.=Zeitschrift für angewandte chemie
 Zeit. anorg. Chem.=Zeitschrift für anorganische chemie
 Zeit. Elektrochemie=Zeitschrift für Elektrochemie
 Zeit. physik. Chem.=Zeitschrift für physikalische chemie

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E. L. Thorndike

EDWARD LEE THORNDIKE

1874-1949

BY ROBERT S. WOODWORTH

At the turn of the century, when psychology as an experimental science was very much in its beginnings but attracting a group of energetic young investigators, an outstandingly energetic, daring and even iconoclastic member of the group was the man who now, at the end of a very active career of fifty years, is recognized as the most productive psychologist our country has produced. Edward Thorndike started young and continued his scientific output until his death at nearly 75 years of age. His full bibliography counts up to over 500 titles of which over 50 are books; and almost everything he wrote was based directly on data, usually on new data; for it was characteristic of him to dislike any abstract discussion not tied closely to concrete facts.

His parents and grandparents were all natives of the state of Maine. His father, Edward Roberts Thorndike, a vigorous, fine-looking man of active mind and generous, impulsive disposition, first practiced law in Maine and then became a Methodist clergyman and a noted preacher in Massachusetts where most of his pastorates were located. He married Abigail Brewster Ladd, described as an extraordinarily intelligent and capable person, of marked artistic ability, deeply religious and of shy, gentle manner but with a will of steel. Her widowed mother, a woman of unusual ability and courage, was a helpful and much-appreciated member of the Thorndike household. Four highly gifted children were reared in this household, all of whom lived to make their mark in the scholarly world: Ashley and Mildred in English literature, Lynn in medieval history, and Edward in psychology and the science of education. Ashley was the oldest and Edward the next in order.

Edward learned to read at home but attended school regularly from the time he turned five years old. Beginning at the age of twelve he attended the high schools of Lowell, Boston and Providence. He made a superior undergraduate record at

Wesleyan University, 1891-95. His interest so far, conforming to his family background, was in literature and "general erudition" and not in science. Going next to Harvard for graduate study, he still made literature his major subject. Meanwhile, as a college junior, he had occasion to study certain chapters of the *Principles of Psychology* by William James and found that book the most stimulating he had ever read. At Harvard, accordingly, he attended James's lecture course, with the result that during that first year he made a decisive shift from literature to psychology. In the second year he undertook as his research project an experimental study of the instinctive and intelligent behavior of young chicks, a topic which he himself suggested though not at the time with any far-reaching plan—"not at all," as he said later, "because I knew animals or cared much for them, but because I thought I could do better than had been done." Since animals were not allowed in the laboratory he did the work in his own room and later in Professor James's cellar. He got results; the experimental method proved fruitful; and when he went to Columbia as a Fellow the following year, he was encouraged by Professor Cattell to continue this line of work. The resulting dissertation on *Animal Intelligence*, 1898, is a landmark in the history of psychology. It inaugurated the laboratory study of animal learning and demonstrated that animal behavior observed under experimental conditions could help solve the general problems of psychology. It was quickly paralleled at Clark University and followed up in many other universities, and the animal laboratory has ever since been an important factor in the development of scientific psychology.

Thorndike delighted to honor James and Cattell as his masters in psychology. Neither of them was responsible for this epoch-making investigation of animal learning nor for his resulting devotion to the experimental method. James was responsible for his initial attraction toward psychology as a natural science dealing with the "rich details of concrete human nature." Cattell was responsible for initiating him into the "quantitative treatment of mental facts," and Franz Boas at Columbia showed him the value of the newer statistical methods. Experiment and

measurement—these two were Thorndike's guiding stars throughout his career.

That first paper of his on *Animal Intelligence* is a landmark in more ways than one. Not only did it inaugurate the animal laboratory in psychology, but it also announced as its major result a new law of learning, additional to the old standard laws of association and important enough, though often combated by other psychologists, to become and remain the focal point of most subsequent discussions of the theory of learning. This new result could emerge because of Thorndike's novel form of experiment. Previous experimental work on learning—some of it excellent—had assigned the learner a poem to memorize, the Morse telegraphic code to master in sending and receiving messages, or some other fixed lesson to be learned. Thorndike placed his chicks, cats or dogs in a problem situation where alternative responses were possible and the first response was unlikely to be successful. The question was whether the animal, perhaps after much trial and error, would learn to do the right thing in the situation—and how rapid his learning might be. For example, a kitten was placed in a slatted box the door of which was loosely bolted, and a bit of fish was placed just outside as a reward. Usually a kitten would not touch the bolt except after making a number of more natural responses. But in a series of trials the unsuccessful responses would fade out and the correct response would occur more and more quickly. The "effect" or outcome of any response was thus a powerful factor in its elimination or establishment. As Thorndike used to phrase it, the law of effect stated that a satisfactory outcome of any response tended to "stamp in" its connection with the given situation, while an unsatisfactory outcome tended to "stamp out" the connection. Whereas previous theories had accepted repetition as the potent factor in learning, Thorndike laid at least equal stress on "effect," i.e., on success or failure, reward or punishment, satisfaction or annoyance to the learner.

Furthermore, the process of stamping in or out appeared from the data on cats and dogs to be gradual and not indicative of sudden insights such as may often occur in man. In a follow-

up study of monkeys (1901) Thorndike found more evidence of rapid learning, but it still seemed probable that what was learned was stimulus-response connections, and that the superiority of monkey to cat, and even of man to monkey, was fundamentally a greater facility in forming connections or associations. This was the germ of his "quantitative theory of intelligence."

After a single year of teaching at Western Reserve University he returned to Columbia in 1899 and undertook the task of developing a scientific educational psychology in Teachers College. He brought to this task his firm conviction of the value of experiment and measurement, and it can fairly be said, as it has been said repeatedly, that Thorndike more than any other man introduced scientific methods into the study of education. In this field, once more, he was the pioneer who opened the way for an immense amount of investigation. The further development of animal psychology he left mostly to others, devoting his own efforts to educational psychology. The law of effect he found directly applicable to education, since it indicated that mere repetitive drill would be ineffective. The child should obtain satisfaction from his correct responses, and his lessons should be such as would enlist a child's interest and awaken a zeal for immediate achievement. By experimental studies of children's specific difficulties in reading, arithmetic, algebra and other subjects Thorndike and his pupils worked out procedures for meeting the child on the child's own ground and for individualizing education.

A major contribution was his attack on the doctrine of "formal discipline," an attack dating from 1901 but repeated at intervals, always with fresh evidence. Traditionally, education was supposed to exercise and develop the mental faculties, so that the educational value of geometry, for example, lay in its power to develop the faculty of reasoning. Thorndike was suspicious of this doctrine from the start. He proceeded to test it by use of the "transfer" experiment. He would give a person intensive training in some narrow field and then test that person's abilities in a more inclusive field and determine how much improvement could be demonstrated beyond the limits of the special

training. The experiment showed on the whole a meager transfer effect which could be explained without the assumption of generalized faculties and abilities. The ability developed by training in one line of work was specific and did not spread to other lines of work except when what had been learned could be utilized in a concrete way. Accordingly he urged that school subjects should be valuable for their content and not merely for drill. He applied this criterion both to the detailed content of each school subject and to the curriculum as a whole. He did not rest his case entirely on this laboratory experiment. Much later (1924, 1927) he published extensive investigations of the after-effects of different high-school subjects. It appeared that the only justification for any subject was the intrinsic value of its subject matter to the student. A good student will gain from any subject that enlists his interest and wholehearted effort. "When the good thinkers studied Greek and Latin, these studies seemed to make good thinkers. Now that the good thinkers study physics and trigonometry, these seem to make good thinkers. If the abler pupils should all study physical education and dramatic art, these subjects would seem to make good thinkers. These were, indeed, a large fraction of the program of studies for the best thinkers the world has produced, the Athenian Greeks."

Both of his early experiments—the one on transfer and the one on animal learning—led up to Thorndike's celebrated theory of the specificity of abilities. A third approach which he soon adopted was statistical rather than experimental; it utilized Karl Pearson's correlation coefficient, then newly invented. If abilities are manifold and specific, an individual can be strong in some while weak in others, even within the range of such a "faculty" as memory or reasoning; but so far as ability is generalized, the correlation between different performances will be high. Large-scale investigations showed low correlations between abilities, as a rule, and so confirmed Thorndike in his specificity theory. He soon encountered strong opposition, however, from the British psychologist, Charles Spearman, who showed how the correlational facts could be interpreted as

indicative of one general ability combined with many specific abilities. So began a controversy which lasted for several decades, enlisting able supporters on both sides, unearthing many important facts, and developing new statistical methods such as are now used in factor analysis. Thorndike adhered to his "quantitative theory" which held that the only general ability was the ability to learn associations or connections. A higher level of ability simply depends on more numerous and more subtle connections. In this as well as other controversies Thorndike revealed one of his most notable characteristics: he would hold up his end with all vigor and assurance, but without any personal animosity. Indeed the contestants joined in a co-operative effort to make sure of the relevant facts.

Very influential in the early days was Thorndike's *Introduction to the Theory of Mental and Social Measurements*, 1904, a textbook based on the view that non-mathematical investigators in psychology and education should be enabled to make intelligent use of statistical methods, understanding the principles involved and appreciating both the great need for quantitative study of human behavior and the inherent difficulties of such an enterprise. Without being himself a highly trained mathematician, he possessed a keen sense for the realities and probabilities. In his own researches he successfully attacked a whole series of ticklish statistical problems. Many of his students joined in promoting the cause of measurement in education: measurement of the individual child's capacity and readiness for the school subjects, measurement of the pupil's achievement, measurement of teaching effectiveness. Thorndike was especially active in the invention and improvement of tests. He worked out scales of achievement in arithmetic, reading, English composition, handwriting, drawing, etc., by which a child's progress could be objectively measured. He insisted on the need for tests scaled in equal units and based, wherever possible, on an absolute zero of achievement in each kind of ability.

The three-volume text on *Educational Psychology*, 1913-14, brought together his major research interests in the period preceding the first World War. The psychology of learning was

given full treatment, starting with the laws of learning derived from his animal experiments and proceeding to a critical analysis of all available data on the improvement of human abilities by training. In the section on the "original nature of man," which was based largely on his personal observations of young children, he sought to escape from the customary vague generalities concerning instincts and to substitute specific statements on innate connections between situations and responses. Another large section of the same book dealt with individual, family, race and sex differences and their dependence on heredity and environment, this being one of his long-continued interests. There was also an extensive treatment of mental fatigue and the work curve, representing an early line of research which he did not continue, except for his study in 1914-16 of the effect of room ventilation on mental work.

During the first World War, while remaining in civilian status, he was one of the most active and effective psychologists who participated in the personnel work of the Army. He contributed to the introduction of the group intelligence tests and tests for aviators. The psychologists of that day made a genuine contribution to the war effort and acquired much new technique and knowledge of testing. Thorndike was one of those who made good use of this experience for civilian purposes after the war. The Thorndike college entrance test was especially famous. Always responsive to the call of duty and opportunity, he engaged in a wide variety of scientific and educational enterprises. As he said in his brief autobiography (1934), "Within certain limits set by capacity and interest I did in those early years and have done since what the occasion seemed to demand . . . Probably it would have been wiser to plan a more consistent and unified life-work . . . but I am not sure."

From 1922 on, however, he was enabled to devote his energies to large-scale investigations in the Institute of Educational Research at Teachers College, with financial assistance from the large Foundations. He was now free to choose his lines of work, and he chose two main lines, mental measurement and learning.

In the work published in 1926 on *The Measurement of Intelligence* he made a determined effort to provide a measuring instrument that would meet the requirement of equal units reckoned from an absolute zero. The technical scaling methods are rather intricate, but the resulting instrument has stood up under continued use and expert criticism. This test scale, the "CAVD," combines four sorts of task: completion, arithmetic, vocabulary, directions. There are 17 levels of difficulty, ranging from the level of the three-year-old child up to that of a very superior adult. The child of three is nowhere near the absolute zero, being already 23 of those equal units above zero, more than halfway up toward the peak of intellectual ability. The peak, according to Thorndike's results, is not reached till late in the teens or early in the twenties, and the decline from the peak is very slow and gradual in the next two decades at least. In his studies of *Adult Learning* (1928) and of *Adult Interests* (1935) he showed that both the ability to learn and the interest in learning something new and valuable were still good enough in the forties to justify adult education as a means of enabling people to keep pace with their changing world.

In his renewed investigation of the process of learning (reported largely in the *Fundamentals of Learning*, 1932, and the *Psychology of Wants, Interests and Attitudes*, 1935) he rechecked his early theory by means of a whole array of novel experiments, designed to separate the results of reward and punishment, of satisfaction and annoyance. The law of effect stood firm in its positive phase but showed weakness in its negative phase. The "stamping in" of successful responses was fully as important as he had ever believed, but the "stamping out" of unsuccessful responses was a much less potent factor. Satisfaction, he concluded, brought into play a "confirming reaction" of the organism, but annoyance did not have a directly opposite effect. Punishment could do good indirectly by inducing the organism to shift from the punished response to some other response which might prove successful; but the learning was due essentially to the confirmation of the successful response. Punishment, as well as reward, could be informa-

tive, but reward and satisfaction operated not only at the conscious level of clear observation and definite selection of the successful responses but also at a much more primitive and physiological level. Reward, he found, could even at times reinforce an unsuccessful act that happened to occur close to a successful act—the “spread of reward” which we might also call the blunderbuss effect. In his subsequent applications of learning theory to educational and social problems, accordingly, Thorndike laid much greater stress on rewards than on punishments. “Except when and as it causes the person to shift to the right behavior and receive a reward therefor, the punishment has no beneficial effect comparable to the strengthening by a reward. . . . Psychology emphasizes the importance of making a community attractive to the able and good rather than unpleasant for those who are incompetent and vicious.” So he wrote in one of his last books, *Human Nature and the Social Order*, 1940.

Thorndike was unwilling to recognize any limits to the scope of quantitative science. In 1903, indeed, he admitted that the aim and goal of education were to be determined “not by facts but by ideals,” and that science was therefore concerned only with discovering the best route to the ideal goal. But his mature view was altogether different. In his 1934 presidential address before the American Association for the Advancement of Science he urged on the scientific world the view that values, being dependent on human wants, were facts of nature, lying properly within the scope of natural science, even though any adequate measurement of human values might be a difficult undertaking. An often quoted statement dates from 1918: “All that exists, exists in some amount and can be measured.” In the 1940 book just quoted he applied this postulate to values: “Any want or satisfaction which exists at all exists in some amount and is therefore measurable, how exactly . . . we cannot tell until we have tried.”

A characteristically extensive research of his later years (1937-1944) attempted to apply to cities the statistical methods that had served him well in the study of individuals. This was

another bold pioneering job. He assembled from the census and other sources the available data on the health, economics, educational and recreational facilities, etc.—nearly 300 items in all—for each of the over 450 cities in the United States having a population in 1930 of at least 20,000. He wished to determine the value of each city as a good place for good people to live. He selected 37 of the items as partial indicators of goodness and constructed a weighted composite of these 37 variables for his general index, *G*, of city goodness. The cities showed a wide and challenging range of *G* values. In the hope of discovering causal factors for this wide variation of *G*, he used the methods of correlation, including partial correlation and path coefficients. Many supposedly important factors, including the mere size of a city, proved to have little influence on *G*, but a large share of the variation, as much as 75-80 percent, could be attributed to the personal qualities of the citizens, to their income level, and to the combination of these two. Aside from the statistical skill and acumen of this investigation, its originality lay in the determined effort to obtain and use some defensible objective measure of the human value of a city.

Another of his latest enterprises might seem to lie outside the field of science but is nevertheless a fine example of his scientific methods. He prepared a series of English dictionaries for school use (1935-1948), ranging in level from the early grades to the secondary school. He had previously made an elaborate word count as a basis for determining which words, because of frequent use, deserved emphasis in the teaching of reading and spelling (*The Teacher's Word Book*, 1921, 1944). His dictionaries were based upon the principle of meeting the child's needs. Words and meanings should be included which were likely, according to the word counts, to be encountered and looked up in a dictionary. The definition (or explanation) of any word should be adapted to the probable background of a child who would look up that particular word. And the definition should never be formulated in more difficult or unusual words than the word defined. Also, the beginner's dictionary should include lessons on how to use a dictionary with satisfac-

tion. This work, now recognized as an important contribution to lexicography, shows the Thorndike traits of experimental and quantitative approach, unending patience, and practical good sense. It illustrates a fact mentioned in a comprehensive review of Thorndike's life work*, that he was "a great tool-maker." Experimental and statistical tools for research, tests and teaching methods for practical application, were among his most distinctive contributions.

Not a few of Thorndike's labors throughout his career were concerned with the psychology of language in its practical aspects such as reading, writing, learning a foreign language, and the frequency in usage of words and their meanings. He did not fail to make scientific use of these labors. Late in life he published some significant studies of phonetics and semantics and offered a psychological theory of the origin and development of language.

That Thorndike was a prodigious worker has been reported by his coworkers and is evident from his vast output. More than that, he was an efficient worker, quick to get started, free from internal friction, unhampered by meticulous pedantry, avoiding unnecessary work. He liked to work intensively and continuously until a job was finished. When he started to read a book—being himself an extremely rapid reader—he liked to finish it at a single sitting. He would relax for a few puffs of a cigarette and resume speed on the task in hand. When he tackled a new problem, he would quickly appraise the possibilities and adopt a tentative plan of attack, feeling perfectly free to shift to a more adequate plan after becoming better acquainted with the intricacies of the problem. The work he liked best was done in his own well-equipped study in long hours free from any danger of interruption.

Yet he was by no means a "lone worker" in the usual sense. He was an excellent collaborator, an effective committee member, responsive to proper demands on his time and sometimes taking the initiative in organizing a working committee. Though

*Florence L. Goodenough, Edward Lee Thorndike: 1874-1949. *Amer. J. Psychol.*, 1950, 63, 291-301.

he disliked speculative debate, he was fair-minded and even-tempered in discussion, tolerant of divergent views though always asking for "the evidence." As the head of a working organization he was considerate and generous of credit to his subordinates who regarded it as a privilege to work with him. As a teacher he showed the same qualities, and though his courses were rather strenuous for the students, his success was shown by the large number who followed his lead as investigators in the educational field.

His preference in social life was for a small circle of intimate friends rather than for any large gathering. The annual meetings of the national scientific societies he found unduly fatiguing. It was Thorndike who took the initiative in assembling a small group of colleagues and moving well out into the country to form the "Montrose colony" of Columbia families. Always a good earner, he made it a practice to spend freely for personal and family comfort and convenience and for other worthy and reasonable causes. As a friend and neighbor—and as a husband and father, the head of an admirable family—he was generous and always dependable, full of cheerful initiative, much loving and much beloved.*

*The high esteem in which Dr. Thorndike was and is held by his immediate colleagues and coworkers is admirably revealed by the appreciative reviews of his work contained in the *Teachers College Record*, 1926, 27, 458-586; 1940, 41, 695-788; 1949, 51, 26-45. These reviews provide the main source of information on his career.

EDWARD LEE THORNDIKE: VITA

- Born: Williamsburg, Mass., August 31, 1874.
Died: Montrose, N. Y., August 9, 1949.
Wesleyan University, Conn.: A.B., 1895; Sc.D., 1919; Trustee, 1934.
Harvard University: A.B., 1896; A.M., 1897; LL.D., 1934.
Columbia University: Ph.D., 1898; Sc.D., 1929; Butler Medal, 1925.
Other honorary degrees: Sc.D., University of Chicago, 1932, and University of Athens, Greece, 1937; LL.D., University of Iowa, 1923, and University of Edinburgh, 1936.
Western Reserve University: Instructor in Education, 1898-99.
Teachers College, Columbia University: Instructor in Genetic Psychology, 1899-1901; Adjunct Professor of Educational Psychology, 1901-04; Professor, 1904-40; Emeritus Professor, 1940-49; Director, Division of Psychology, Institute of Educational Research, 1922-40.
Marine Biological Laboratory of Woods Hole: Head, Department of Comparative Psychology, 1900-02.
Cornell University: Messenger Lecturer, 1928-29.
Yale University: Lecturer, Summer 1931.
Johns Hopkins University: Visiting Professor, 1931-32.
Teachers College, Columbia University: Kappa Delta Pi Lecturer, 1939.
Harvard University: William James Lecturer, 1942-43.
New York State Commission on Ventilation, 1913-22.
United States Army: Chairman, Committee on Classification of Personnel, 1917-18, and Member of Advisory Board, Division of Psychology, Office of the Surgeon General, 1917-18.
American Council on Education: Committee on Problems and Plans.
Motion Picture Research Council: Member of National Committee.
Carnegie International Examination Enquiry, 1938.
Lexicographic Advisory Committee, 1936-1941.
National Academy of Sciences: Member, 1917.
American Philosophical Society: Member, 1932.
American Academy of Arts and Sciences: Fellow, 1934.
New York Academy of Sciences: Fellow, 1901; Vice President, 1902; President, 1919-20.
American Association for the Advancement of Science: Fellow, 1901; Vice President, 1911, 1917; President, 1934.
American Psychological Association: President, 1912.
Psychometric Society: President, 1936-37.
American Association for Adult Education: President, 1934-35.
Society for the Advancement of Education.
Society of Naturalists.
Galton Society.
American Economics Association.
American Sociological Society.

British Psychological Society: Honorary Member, 1926.

Leningrad Scientific-Medical Pedological Society: Honorary Member, 1928.

Comenius Educational Association of Czechoslovakia: Honorary Member, 1929.

Phi Beta Kappa.

Sigma Xi.

Kappa Delta Pi.

Cosmos Club, Washington.

Century Association, New York.

Married, 1900, Elizabeth Moulton. Children: Elizabeth Frances, a mathematician; Edward Moulton, a physicist; Robert Ladd, a psychologist; Alan, a physicist.

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KEY TO ABBREVIATIONS

Amer. J. Hyg. = American Journal of Hygiene

Amer. J. Psychol. = American Journal of Psychology

Amer. J. Pub. Health = American Journal of Public Health

Amer. J. Sociol. = American Journal of Sociology

Amer. Nat. = American Naturalist

Ann. N. Y. Acad. Sci. = Annals, New York Academy of Sciences

Arch. Philos. Psychol. Sci. Meth. = Archives of Philosophy, Psychology and Scientific Methods

Arch. Psychol. = Archives of Psychology

Brit. J. Educ. Psychol. = British Journal of Educational Psychology

Columbia Univ. Contrib. Philos. Psychol. Educ. = Columbia University Contributions to Philosophy, Psychology, and Education

Comp. Psychol. Monogr. = Comparative Psychology Monographs

Educ. Forum = Educational Forum

Educ. Rec. = Educational Record

Educ. Rev. = Educational Review

Elem. School J. = Elementary School Journal

Eug. News = Eugenical News

Harvard Educ. Rev. = Harvard Educational Review

Internat. Mo. = International Monthly

- J. Abnorm. Soc. Psychol. = Journal of Abnormal and Social Psychology
- J. Adult Educ. = Journal of Adult Education
- J. Amer. Statist. Assoc. = Journal, American Statistical Association
- J. Appl. Psychol. = Journal of Applied Psychology
- J. Bus. Educ. = Journal of Business Education
- J. Educ. Psychol. = Journal of Educational Psychology
- J. Educ. Res. = Journal of Educational Research
- J. Exp. Psychol. = Journal of Experimental Psychology
- J. Gen. Psychol. = Journal of Genetic Psychology
- J. Hered. = Journal of Heredity
- J. Ped. = Journal of Pedagogy
- J. Philos. Psychol. Sci. Meth. = Journal of Philosophy, Psychology and Scientific Methods
- Math. Teach. = Mathematics Teacher
- Mech. Eng. = Mechanical Engineer
- Nat. Acad. Sci. Biogr. Mem. = National Academy of Sciences Biographical Memoirs
- Nat. Soc. Study Educ. = National Society for the Study of Education
- Ped. Sem. = Pedagogical Seminary
- Pop. Sci. Mo. = Popular Science Monthly
- Proc. Amer. Philos. Soc. = Proceedings, American Philosophical Society
- Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences
- Psychol. Bull. = Psychological Bulletin
- Psychol. Clinic = Psychological Clinic
- Psychol. Rec. = Psychological Record
- Psychol. Rev. = Psychological Review
- Pub. Op. Quart. = Public Opinion Quarterly
- Quart. J. Econ. = Quarterly Journal of Economics
- Quart. J. Speech = Quarterly Journal of Speech
- School Rev. Monogr. = School Review Monograph
- School Sci. Math. = School Science and Mathematics
- School Soc. = School and Society
- Sci. Mo. = Scientific Monthly
- Teach. Coll. Rec. = Teachers College Record
- U. S. Bur. Educ. Bull. = United States Bureau of Education Bulletin
- Univ. Calif. Chron. = University of California Chronicle

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J. B. Leitch

FRANK BALDWIN JEWETT

1879-1949

BY OLIVER E. BUCKLEY

A statesman has been defined as one who shows unusual wisdom in treating or directing great public affairs. Among scientists and engineers there are few to whom this broad definition may be more appropriately applied than to the late Frank Baldwin Jewett, President of the National Academy of Sciences from 1939 to 1947. It was fortunate indeed that among the members of the Academy there was one so well qualified by disposition and experience to assume leadership of that body during the exacting times that followed his election. To his fellows and to the nation his service as president of the National Academy was the climax of a career of devotion that has left its enduring mark on the history of an epoch in the development of applied science.

Jewett came from a long line of New England ancestors, the first of whom had settled in Rowley, Massachusetts, in 1632. Many of the later generations played prominent roles in colonial New England, but his immediate forbears had settled in Ohio where his father, Stanley P. Jewett, and his mother, Phebe C. Mead, were living at the time they were married. A few years earlier, an uncle of his father had been one of a group that purchased the entire area of what is now Pasadena and Altadena, north-easterly of Los Angeles. This was largely unsettled and undeveloped country, and even Los Angeles itself at that time had a population of under 10,000. As an inducement to settle in and develop southern California, Jewett's grandmother, Belle Maria Stanley Jewett, purchased a twenty-five acre tract of this land and offered it to Jewett's father as a wedding present. The property was in Pasadena—a community at that time of some twelve houses. It was here that Frank B. Jewett was born, the elder of two children, on September 5, 1879.

Muscat grapes and oranges were the major crops, and raisins and wine were the chief products of this pioneering community. Jewett's father, however, was an engineer, and had attended Massachusetts Institute of Technology before his marriage. Although he actively managed his ranch, his heart was in engineering—particularly railroading, in which he had been engaged in Ohio. In the middle '80's he and two others organized the Los Angeles and San Gabriel Valley Railroad, and built a line from Los Angeles to Pasadena. This is now the main line of the Santa Fe, to which it was later sold, and of which Jewett's father later became a vice president. Jewett was fascinated by railroads as a result of his experiences with them during these early years, and until he was in college had intended to make railroading his profession.

By the time Jewett was eight, his father had sold their property in Pasadena and moved to Lamanda Park—some five miles directly east of Pasadena. Here Jewett attended a one-room, one-teacher school until he finished the eighth grade. He then went to the preparatory school of the recently founded Throop Institute of Technology, which has since become the California Institute of Technology. He later attended the Institute itself, and received the A.B. degree in 1898, when he was only nineteen years old.

During his first years at Throop, he had ridden back and forth each day on horseback. The morning, noon, and evening care of the horse that this entailed, however, was not entirely to his liking, and he later changed to a bicycle—first one of the high wheel affairs, but later one of the "safeties," then in their infancy. During his last year or two, he and his sister Pauline, who also attended Throop, drove to school. During all these early years, he lived an outdoor life and was subject to all the subtle influences of space and color that a seemingly endless expanse of desert and mountains can exert on a young, imaginative, and observing mind. He helped with the work in the apricot and orange groves, and at other chores around the place, but there was plenty of time for hunting and fishing, which were at that time entirely untrammelled by game laws

and other restrictions.

In the meantime, his father had left the Santa Fe and had become interested in the power industry, which in the late 1890's had begun to install street railways and arc lights in fast-growing southern California. Jewett's was one of those versatile minds that become interested in whatever turns up to be done, and at his father's instigation he had planned to study electrical engineering to assist his father in this new field of interest. Since Throop was a small college, and there was thus the feeling that perhaps Jewett was not able to obtain from it the best instruction and the latest knowledge in the new and growing science of electrical engineering, it was planned that he should go to the famous M.I.T. for graduate study before assuming his electrical engineering responsibilities with his father. He had planned to enter Tech in the fall of '98, but his mother's death and his father's temporary absence from California required that he remain in Lamanda Park. It was the end of the year before he was free, and then it was too late to enter M.I.T. that year.

His former physics professor at Throop happened to be taking a sabbatical at the University of Chicago, and wrote Jewett to come there to take graduate work in physics. The proposal appealed to both Jewett and his father, and as a result in January, 1899, he entered the University of Chicago, which had only recently been founded and was blazing new paths in education.

John Mills, who was later associated with Jewett in the American Telephone and Telegraph Company, was an undergraduate at Chicago when Jewett came there to take graduate work, and since they were both working in physics, they became acquainted. At the time of Jewett's retirement, Mills wrote:

"Despite his youth Jewett was one of the most mature-minded of the graduate students at Chicago; and he associated on terms of congenial informality with professors and instructors as well as with his fellow students. A small group of the latter he organized for extra-curricular reading of mathematical physics. Not all his time, however, was spent in study and laboratory

work for he was a leading spirit in a small and convivial group of graduate students whose bimonthly symposia were not entirely devoted to serious conversation. He also joined and was active in one of the national fraternities, Delta Upsilon. More importantly, he became engaged to Fannie Frisbie, a young graduate student in physics, whom he married a few years later."

His marriage in 1905 was a most happy one, marked not only by unusual devotion but by rare intellectual companionship. The death of Mrs. Jewett on December 17, 1948 was a deeply felt loss that perhaps hastened his own death, which followed within less than a year. Three children had been born to them: a daughter who died in infancy, and two sons who survive them: Harrison Leach, attached to the Brookhaven National Laboratory, and Frank Baldwin, Junior, of the General Mills Company of Minneapolis.

As a graduate student at Chicago, he served for a short time as research assistant to Prof. A. A. Michelson, and received his doctor's degree in 1902. Jewett next went to M.I.T. as an instructor in physics. Probably his decision some years earlier to attend M.I.T. had something to do with this move to Boston.

One day in the spring of 1903, during Jewett's first term at M.I.T., Dr. George A. Campbell of the engineering department of the A. T. & T. visited Professor Harry E. Clifford of M.I.T. to inspect a high-frequency generator that had just been acquired by the Institute for some experimental studies. While Campbell and Clifford were talking, Jewett happened to pass by, and Clifford introduced him to Campbell.

Campbell, who was in charge of the group concerned with loaded lines and the theory of telephone transmission, was on the lookout for young engineers and scientists to assist in solving the many difficult problems that faced the engineering department at that time. Jewett created a favorable impression immediately, and after he had left, Campbell asked Clifford about the possibility of employing him. On being told that Jewett was under contract with the Institute for another year, Campbell dropped the subject for the time being. Within an-

other ten or twelve months, however, Campbell raised the question again, and as a result Jewett visited the A. T. and T. engineering headquarters in Boston. He created the same favorable impression on the executives of the engineering department that he had on Campbell, and a few days later it was agreed that Jewett would join the A. T. and T. at the end of his 1904 term at Tech. His starting salary was \$1600 a year which at that time was very high for a man even of Jewett's training and experience.

In 1904 when Dr. Jewett began his association with the American Telephone and Telegraph Company, the telephone system had already made considerable growth, but it was still far from being able to give nationwide, to say nothing of worldwide, communication. Commercial telephone service over open-wire lines between New York and Chicago had been available for only three years. There were no long cable circuits such as now interlink all the major cities across the entire width of the country; there was no radiotelephony; and there was no broadcasting. A way had been devised to transmit three telephone conversations over two pairs of conductors by that time, but the means by which today we transmit 800 conversations over a single pair of conductors, and our present ability to talk, to telegraph, to send photographs, and even to transmit the living scene by television from almost any place in the world to any other, were still for the future to reveal. Of greater significance to the role Jewett was to play in future developments, however, was the fact that the knowledge, the techniques, and the tools that would make these advanced forms of communication possible had not yet come into existence.

One of the many and varied problems facing the Bell System at this time was the protection of telephone lines from noise and high voltages induced by nearby electric railways. This was among the major projects on which Jewett worked during his first few years. There were plenty of others, however, and one of them—a study and report of a recently issued patent—played a significant part in his advancement. The patent covered a number of schemes for improving transmission over long

telephone lines. Jewett's report was based on both theoretical studies and experiment, and was so clear and fair that it attracted the attention of F. P. Fish, president of the A. T. & T. Company. In this report, as in Jewett's other early work, there were evident the keen analytical ability, the refusal to be blinded by superficial obscurity, and the perception of essentials that were characteristic of him throughout his life.

There were other characteristics of Jewett, however, that also became evident even in these early years. He recognized that the success of Bell System engineering in the years to come would depend to a large extent on the type of men brought into the System to carry out its program. As early as 1905 he laid the foundation of a splendid record as a personnel recruiting agent for the headquarter's staff, and many of the men he brought into the Bell System made signal contributions to its technical and material growth. His ability to select men who could ably direct and correlate the work delegated to them was largely instrumental in securing success in later years when only matters of broad general policy came under his direct supervision.

Jewett's first position with the A. T. & T. Company, in 1904, was as a transmission engineer under Campbell. By January 1906, he had succeeded Campbell as head of the electrical department in Boston. Late in the following year, the engineering department was moved to New York under John J. Carty, chief engineer, and with Jewett as transmission and protection engineer.

Late in 1908, Mr. Carty and a number of his assistants, one of whom was Jewett, made a trip to the Pacific coast to review the telephone situation there. Preparations were already under way for the Panama-Pacific Exposition—originally scheduled for 1914, but later postponed to 1915—and businessmen were urging that telephone communication with the east be ready for its opening. Carty had felt the absence of daily telephone conversations with his staff in New York, and he remarked to Jewett that he "was greatly impressed with the isolation of the rest of the country." Transcontinental te-

lephony had always been an objective for the future, and in view of the pressure for its prompt realization, Carty and his staff spent long hours while on the coast studying the problems to be overcome in making it possible. Success seemed attainable, and before the group left the coast, President Vail, who had later come from New York to join them, told the management of the Exposition that he would attempt to complete a transcontinental line in time for the opening. The responsibility for meeting this promise was placed primarily on Jewett. It was the first large undertaking in which he demonstrated his abilities, and is typical of much of the work he did later.

One of the essential requirements for a transcontinental line was a satisfactory repeater: a device that would receive speech waves after they had been attenuated by passing over a line, and amplify them for further transmission. A repeater had been developed by the time Jewett joined the Bell System, and its first successful trial was made that year. This first repeater was of the mechanical type, and consisted of a telephone receiver placed face to face with a carbon transmitter—a single diaphragm being common to both. Through the winding of the receiver, incoming speech currents actuated the diaphragm and resulted in amplified speech at the output of the transmitter. Although this repeater had considerable use in the years following 1904, it had serious inherent weaknesses. As Jewett pointed out in a paper presented with Bancroft Gherardi before a joint meeting of the A.I.E.E. and the I.R.E. in October 1919: "The amplification is not, however, sufficiently independent either of the amplitude or of the frequency of the input to make them adaptable with best results to use in tandem, although they have been used to advantage on telephone lines the length of which did not require more than three repeater stations." This meant that telephone lines could be lengthened only until their increased losses equaled the gain of three repeaters. When it is realized that our present transcontinental circuit may employ as many as 400 repeaters in tandem, the seriousness of this restriction is evident.

Although by the use of large-size conductors and the application of loading it had been possible to provide usable telephone circuits between New York and Chicago, and a project was under way to extend this circuit to Denver, Jewett had long recognized that it would be economically unsound to attempt to reach San Francisco without an entirely new type of repeater. In a memorandum of December 10, 1910, he wrote:

" . . . I am more than ever impressed with the very great need for producing a satisfactory repeater for operation on loaded lines if we are to establish a truly universal service on the North American continent on a paying basis as well as one of true economy.

"From a preliminary study of the situation, I feel very confident that if this repeater matter is tackled in the proper manner by suitably equipped men working with full coordination and under proper direction the desired results can be obtained at a relatively small cost. I feel, however, that to achieve this result it will be necessary to employ skilled physicists who are familiar with the recent advances in molecular physics and who are capable of appreciating such further advances as are continually being made, also that the work must be carefully supervised by someone having a full understanding of the requirements."

Early in the following year, he outlined more specifically what would be required:

"Nature of Work"

"A general study to determine the proper characteristics for the best telephone repeater, its circuit, and the general terminal and line conditions that must be fulfilled to make this repeater available for both loaded and non-loaded lines. This study will include—

"(1) A complete study of the characteristics of the existing receiver-transmitter type of repeater with a view to determining whether the action of this repeater cannot be improved upon and whether modifications in the repeater element, its circuit or in the line conditions will make it suitable for general use on loaded lines.

"(2) A study of other possible repeater ideas, particularly in the domain of molecular physics. Certain characteristics of discharge of electricity through gases and vapors seem to offer considerable possibility of obtaining a telephone amplifier that

will be suitable for use on loaded or non-loaded lines and which will give the desired amplification without a great deal of distortion.

"(3) A mathematical and laboratory study of two-way repeater circuits with a view to determining the best form of repeater circuits to be used in combination with any desired repeater element and any kind of loaded line.

"(4) A mathematical and experimental investigation of loaded line characteristics in the existing plant, and a determination of what changes, if any, must be made in the construction and installation of loading coils and cables in order to make loaded lines suitable for the application of telephone repeaters."

Jewett's active work in recruiting during these early years has already been referred to. In an effort to secure "skilled physicists who are familiar with the recent advances in molecular physics" of the above quotation, he approached Professor R. A. Millikan of the University of Chicago, with whom he had formed a close friendship during his years at Chicago. In his autobiography,* Dr. Millikan writes of him: "He (Jewett) was younger than I by nine years, and was taking his doctorate at the University of Chicago on a problem suggested by Professor Michelson, namely, the pressure-density relations of sodium vapor at temperatures up to the boiling point of sodium. We boarded together and developed a friendship which was destined to last throughout life."

In regard to Jewett's search for someone to undertake development work on a telephone repeater, Millikan wrote:

"It was in the fall of 1910 that Jewett, with whom I had kept in close contact since he took his doctorate in 1902, came from New York to see me at Chicago. . . . He started our conversation as follows: 'Mr. John J. Carty, my chief, and the other higher-ups in the Bell System, have decided that by 1914, when the San Francisco Fair is to be held, we must be in position, if possible, to telephone from New York to San Francisco. With the aid of the Pupinized lines we have been able thus far to talk between New York and Chicago, but to get through to San Francisco by the present methods is out of the question. The cost is completely prohibitive. We have

* Reprinted with the permission of the publishers from THE AUTOBIOGRAPHY OF ROBERT A. MILLIKAN, Prentice-Hall, Inc.—New York, 1950.

got to develop somehow a telephone repeater that will boost up the speech currents when they become too attenuated, just as the enfeebled currents in dot-dash telegraphy are now boosted up by the repeater on a long telegraph line. You have been working ever since we were together from 1900 to 1902 on these pure electronic discharges in your very high vacua which you were developing at that time. It seems to me that these essentially inertialess electronic discharges are the best bet there is for a telephone repeater. This will be a very difficult task, that of following all the modulations of speech and boosting up their intensity without in any way distorting them. We want you to help us in this job, as follows: Let us have one or two, or even three, of the best of the young men who are taking their doctorates with you and are intimately familiar with your field. Let us take them into our laboratory in New York and assign to them the sole task of developing the telephone repeater. . . . The first man whom I thus sent to the laboratory at 463 West Street, New York, to attack this problem was Dr. H. D. Arnold."

It was Arnold who within a very few years developed the high-vacuum electronic tube from the original deForest audion, and thereby laid a considerable part of the foundation not only for a transcontinental line but for all long distance communications since that time.

Much was needed besides the repeater, however. The open-wire lines required correction; the circuit associated with the repeater needed further invention and refinement to enable it to work both ways; and there were important problems of stability and control. In all this work, Jewett played an important part, although he made few direct technical contributions. He had moved too rapidly into executive work to have time for detailed technical studies. His part was to foresee needs, to perceive the most productive paths, to select, encourage, and stimulate those doing the detailed creative work and to guide all parts of it to a final consummation.

After Jewett's move to New York in 1907, his prestige continued to rise, and in recognition of his capable direction of the transcontinental line and other important engineering projects, he was made assistant chief engineer of the Western Electric Company in 1912.

Since 1881, the Western Electric Company had been the manufacturing organization of the Bell System, and it had a capable staff of engineers and inventors. Its engineering department occupied the building at 463 West Street, New York City since 1900, and had played a large part in developing the apparatus for the rapidly growing telephone system.

Beginning active direction of the research and development work of the Western Electric Company as assistant chief engineer in 1912, Jewett became chief engineer in 1916, and vice president and director in 1921. The engineering department of the Western Electric was much larger than that of the A. T. and T. Company, and it included extensive laboratories in which new apparatus and circuits could be tested, and where organized research was carried on. The two engineering departments had always worked in close cooperation. In general, the technical needs of the Bell System were determined by the A. T. and T. engineering department, which also carried out many of the theoretical studies leading to improved systems and apparatus. The engineering department of the Western Electric, on the other hand, carried out the development and design of the actual circuits and apparatus, and furnished drawings and specifications for the manufacturing department.

In 1924, the engineering department of the Western Electric Company was incorporated as Bell Telephone Laboratories. The legal steps were completed during the closing days of the year, and the new corporation with Jewett as its president began functioning in the opening days of 1925. At the same time, Jewett was made vice president of the A. T. and T. Company. Reporting to him in this latter capacity was Dr. E. H. Colpitts, who was in charge of the department of development and research. This department, together with the A. T. and T. engineering department, was the continuation of the group in Boston with which Jewett was associated when he joined the Bell System. In a period of just over twenty years, Dr. Jewett, after wide supervisory experience in the engineering departments of both the A. T. and T. and the Western Electric Company, had become—as vice president of the A. T. and T.

and as president of Bell Telephone Laboratories—the director of all research and development of the Bell System. In 1934, as the final move in consolidating the research and development of the Bell System, the development and research department was made part of Bell Telephone Laboratories, of which Dr. Colpitts had been made executive vice president during the preceding year. In 1940, Jewett relinquished the presidency of Bell Telephone Laboratories to become chairman of its Board of Directors, which position he retained until his retirement in 1944. His death occurred on November 18, 1949 at the Overlook Hospital in Summit, New Jersey.

In recognition of his many services to the Bell System, and as an endorsement of his farsightedness in stressing the need for fundamental physical research as a basis for successful industrial development, the American Telephone and Telegraph Company established the Frank B. Jewett Fellowships in the Physical Sciences at the time of his retirement. These are post-doctorate fellowships intended to stimulate and to assist research in the fundamental physical sciences, and particularly to provide their holders with opportunities for individual growth and development as creative scientists. They enable their recipients to devote themselves to research in pure science for a year or two following their doctorates.

Jewett's life was almost exactly contemporary with the first seventy years of the telephone. Bell's first patent was issued in 1876, only three years before Jewett's birth, and the Bell Telephone Company was formed in 1877. In 1881, the Western Electric Company became the manufacturing unit of the Bell System. The year 1885 saw the incorporation of the A. T. and T. Company, but only as a subsidiary to handle the long distance business; it was not until 1900, while Jewett was taking graduate work at Chicago, that it became the parent company. The year Jewett joined the A. T. and T. Company, there were fewer than two million telephones in the Bell System, but during the forty-year period he was with the Bell System, nearly 20,000,000 telephones were added—more than ten times the number in the Bell System when he joined it. The figure

might have been larger but for the drastic curtailment in telephone installations made necessary between 1941 and 1944.

This growth in the number of telephones, however, is but one indication of the great expansion of communication facilities that occurred during Jewett's association with the Bell System. In 1904, the longest long-distance call that could be made was from Boston to Chicago, and there was only a single open-wire circuit over which such calls could be placed. In 1944, when Jewett retired, any Bell System telephone in the country could be connected with telephones in almost every country on the globe. Speech was being carried underground in cable, where it was protected from all the hazards of snow, sleet, and wind, beneath the sea in submarine cable, and through the ether of space. Besides carrying voice waves, Bell System circuits now provide teletypewriter, radio broadcast, and television channels not only over open-wire lines and lead-covered cable, but by coaxial and radio relay.

The year in which transcontinental telephone service was initiated also witnessed the first transatlantic telephone transmission. Telephone communication between ship and shore was demonstrated in 1916, and between plane and ground in 1917. Two years later, commercial radiotelephone service was being given between Los Angeles and Catalina Island, and the same year brought forth the first commercial public address system. As a form of communication, radio broadcasting had always been of interest to the Bell System, and their pioneer station WEAJ set many precedents in methods and apparatus that have been followed ever since. The first radio broadcasting wire network was set up in 1923.

Studies of the recording and reproduction of sound in the Laboratories led to the orthophonic phonograph in 1925, and in the following year to the first commercial demonstration of synchronized sound and scene—in the Vitaphone—which paved the way for sound pictures. These studies in sound and speech transmission and reproduction were undertaken, of course, primarily with the objective of improving telephone communication. Of necessity they were far ahead of their economic

application to commercial telephone service, and were climaxed in 1933 by the demonstration of the essentially perfect reproduction of orchestral music in auditory perspective. As Jewett said at the Philadelphia demonstration: "What has been demonstrated here today is perfection in the quality of the electrical pick-up, amplification, and reproduction of orchestral music with apparatus which can be arranged to give substantially perfect auditory perspective. There has also been demonstrated the ability of this apparatus to produce a range in volume, without impairment of quality, far beyond the normal possibility of direct orchestral execution."

In 1927, television transmission by both radio and wire lines was demonstrated, and within a few years the foundations were laid for the coaxial cable system that now forms the backbone of the television broadcasting network. Studies of waveguide transmission at the Laboratories during the early 1930's made techniques available that were of inestimable value in the development of radar during the war.

The summary of Jewett's work has so far been restricted to that in the Bell System, but his accomplishments were not limited to that field. Within less than fifteen years after joining the Bell System, he was achieving a nation-wide reputation. As a Lieutenant Colonel in the Signal Corps during the first World War, as an advisory member of the Special Submarine Board of the Navy, and as a member of the Special Committee on Cables of the State Department, he contributed extensively to the successful prosecution of that war, and was awarded the Distinguished Service Medal in recognition of his achievements. In 1918, he was elected to membership in the National Academy of Sciences, and thus became permanently associated with the scientific needs of the government for both peace and war.

This association became still closer in 1923 when he was made chairman of the Division of Engineering and Industrial Research of the National Research Council. In 1933, he was made a member of President Roosevelt's Science Advisory Board. His great abilities in organizing and directing scientific

research were recognized in his election in 1939 as president of the National Academy of Sciences. He was the first engineer to be elected to this office, the first whose reputation was as a leader of applied research rather than as a research scientist in his own right. There was unrest in the world, and our government was in need of immediate advice on how most quickly and effectively to apply the scientific knowledge and personnel of the country to the urgent needs of national defense. For such a task Jewett was particularly suited. His wide acquaintance among scientists of distinction in all fields, coupled with his rare understanding and his winning personality made it possible for him to assemble committees to deal with every question directed to the Academy from the various government agencies, including such far-reaching ones as atomic fission and measures of defense against biological attack. When his first four-year term as president of the Academy expired in 1943, we were in the midst of conflict, and there was little question in the minds of the members as to who should fill the office for the ensuing term. Jewett was the inevitable choice, and he thus served the Academy and the nation through perhaps the most critical eight successive years in the country's history.

As Jewett pointed out in his review of National Academy activities for the years of his presidency, 1939-47, practically the entire interval was concerned with preparation for war, the war years, and the early postwar period. To a large extent the Academy, during this time, ceased to function as a learned society in the traditional sense, and functioned in its official capacity as the top scientific advisory agency of the Government. Jewett's role in the Academy is well set forth in a letter dated May 8, 1950, by Prof. Ross G. Harrison as follows:

"Jewett's unusual personality and character, together with experience in affairs and his technical know-how in the application of science, combined to inspire confidence in government officials and officers in the armed forces at high level, so that the Academy and Research Council became far better known to them and were looked to for advice and assistance in a measure not before attained. These obligations necessitated a new type of contract with the government offices, including pro-

visions for general overhead expenses, which first required great patience to negotiate before a satisfactory formula could be arrived at. A successful appeal to the Carnegie Corporation for a rotating fund to be used as 'working capital,' made it possible for the Academy and Council to begin preliminary work on projects long before the contracts were actually signed.

"Jewett's sympathetic understanding of the relations between the Academy and the Research Council, and his wholehearted agreement with the Chairman of the Council that everything should be done to eliminate all sources of friction between the two bodies, have done much toward making the two organizations into a harmonious whole, working in cooperation and not in rivalry.

"Among his contributions to the work of the Academy was the reorganization of the finances, particularly the investment policy of the Academy, through the appointment of Mr. Horace Ford as Investment Counsel and the establishment of a group of Special Financial Advisors, who meet from time to time with the Finance Committee for the consideration of over-all investment policies. The finances of the Academy have thereby been greatly strengthened. Another was the establishment of a business office, headed by a competent Business Manager."

Jewett's work with the Academy was only one of the fields in which his active intelligence found scope. With the impact of war, it soon became evident that a new kind of agency would have to be created to make the full potentialities of the scientific talent of the nation available to the military services. The Academy was so constituted that it could not act as a contract agency to expend Government funds for research and development. Recognizing this fact, Jewett was one of the small group of scientists primarily instrumental in creating, in 1940, the National Defense Research Committee, and he became chairman of its Division C concerned with communication, transportation, and submarine warfare. In 1941, he was made a member of the Coordination and Equipment Division of the Signal Corps, and in 1942, consultant to the Chief of Ordnance.

In addition to his governmental and military services, he took part in the work of many organizations of non-military nature. He became vice chairman of the Engineering Foundation in 1919, president of the A.I.E.E. in 1922, and shortly thereafter became a member of the Committee on Scientific

Aids to Learning of the National Research Council. He also was made president and trustee of the New York Museum of Science and Industry, a life member of the Massachusetts Institute of Technology Corporation, a member of the Advisory Committees of the U. S. Departments of Agriculture and Commerce, and a trustee of Princeton University, of the Carnegie Institution of Washington, and of the Woods Hole Oceanographic Institution. He was a fellow of the American Institute of Electrical Engineers, of the American Physical Society, the Acoustical Society of America, the Institute of Radio Engineers, and of the American Academy of Arts and Sciences, and a member of the American Philosophical Society, and the Institution of Electrical Engineers (British).

As a result of the wide recognition of his abilities and achievements, he was showered with honorary degrees and scientific awards. He was made a Doctor of Science by New York University and Dartmouth in 1925, by Columbia and the University of Wisconsin in 1927, by Rutgers in 1928, by the University of Chicago in 1929, by Harvard in 1936, by the University of Pennsylvania in 1940, and by Boston University in 1944. He was given the Doctor of Engineering Degree by the Case School of Applied Science in 1928, and the degree of LL.D. by Miami University in 1932, by Rockford College in 1939, by Norwich University in 1944, by Yale University in 1946, and by the University of California in 1948. He was awarded the Edison Medal in 1928, the Faraday Medal (British) in 1935, the Franklin Medal in 1936, the Washington Award in 1938, the John Fritz Medal in 1939, and the Medal for Merit in 1946. The Hoover Medal and the Industrial Research Institute Medal were awarded him in 1949 and were to have been presented early in 1950. These various honors are eloquent and irrefutable testimony of his impact on society and of the effective results he obtained in his many fields of endeavor.

One of Jewett's many interests outside the scope of his nation-wide responsibilities was the promotion of scientific cooperation between this country and Japan. He quickly

recognized the scientific and industrial competence of the Japanese people, and felt that besides being eager for assistance from us, they also had contributions to make to our own advancement. He became actively interested in Japan's behalf at the time of the Tokyo earthquake in September 1923, and rendered active and effective assistance for which he was later awarded the Fourth Order of the Rising Sun. In 1929, he presented a paper, jointly with Bancroft Gherardi, at the World Engineering Conference in Tokyo, where he had the opportunity to meet many Japanese leaders in science and industry. This was the beginning of a long association between Jewett and Japanese scientists with mutual advantages. As further recognition of his services to Japan, he was awarded the Third Order of the Sacred Treasure in 1930. Later, as president of the National Academy of Sciences, he was instrumental in organizing two scientific missions to Japan during the postwar years. In recognition of his many contributions to Japanese science, a memorial service was held by the Japanese in his honor at G.H.Q. Chapel Center in Tokyo on December 11, 1949. Over 200 scientists and friends were present.

None who has commented on Jewett in the past has failed to record his charm, his warmth, his ability to make friends of people readily and quickly, and at the same time to gain their confidence. Regarding this aspect of his character, John Mills wrote:

"It was in those maturing years at Chicago that Jewett began to display his amazing talent for friendship, an aptitude which has made him so rich in friends and endeared him to so many. To a character marked by frankness and sincerity, to a keen intellect with wide interests and human sympathies, to a sprightly manner and pleasant wit, nature had added a voice which expressed all those characteristics. The voice is particularly effective over the telephone, as anyone can testify who ever heard him say, 'This is Frank'; from that moment on, whether the message it delivers is pleasant or not, loyalty has been aroused. While its modest possessor may have thought

that what was compelling was the idea, the compulsion was always aided by the sunny tone."

Writing of the early Boston years, he said:

"Here again his natural and unconscious ability to make friends rapidly enlarged his circle of influence. In a city where the formalities of social intercourse were strictly maintained and the informality of first names was something to be acquired by years of association, or to be born into, Jewett soon established many intimate friendships. The operation was essentially spontaneous—a few weeks, not months, of acquaintanceship would lead to first-name intimacy. He never presumed nor 'climbed'; his friendships always seemed the spontaneous result of natural affinity. They were just as natural with those of lower rank; and statistically more important because as he progressed the number below increased and that above decreased.

"Whatever the mechanism and however deep the subconscious motivation, this capacity for friendship has been one of the greatest assets to Jewett's career."

This characteristic of graciousness, and the capacity for making friends were among the elemental traits with which Jewett was endowed. His keenness of mind, the ease with which his intellect became interested in almost any problem or situation presented to it, and the logical precision with which his highly rational thought carried long and involved trains of reasoning to their only valid conclusion, these qualities go a long way toward explaining Jewett's rapid rise to positions of the greatest responsibility and his considerable influence within the Bell System, on national affairs, and throughout the scientific world.

There is no answer to the why or how of such a capacity for friendship. It was merely one of the outward marks of the inner personality that was Jewett. One can have no greater asset. It opens doors, and it elicits sympathy, assistance, and cooperation; it engenders the trust and confidence that is essential to great accomplishment. Those who possess it are blessed immeasurably, and there is no doubt that Jewett was one of them.

Kindness was his constant companion, and indubitably contributed to the "amazing talent for friendships" credited to

him by Sir Frank Gill in "Nature" for December 17, 1949. Gill continued: "Those working under his direction did so, not only because his direction was good, but also because it was a labour of love for the leader. He gave an extraordinary example of how to manage men and how to weld together teams of persons (even difficult personalities) into efficient units."

Jewett's relationship to the telephone industry is an outstanding example of a situation needing a man and a man finding a career for which he was peculiarly fitted. The telephone industry was not an activity that Jewett had always dreamed of entering and for which he had particularly prepared. To the extent that he allowed his interests to be confined and focused in his earlier years, their objective was railroading or electric power. The abilities to plan, to organize, and to accomplish were his, but they could have been applied with success in any of a number of fields. The finger of fate, however, seems steadily and insistently to have directed him to the path we have found him following.

Chance prevented him from entering M.I.T. in the fall of 1898 to study power engineering as he had planned. Instead fate led him to Chicago and to four years of physics. This done, he picked up the thread of his earlier intention and went to M.I.T., but here again fate was subtly at work, because it was in Boston that the engineering department at A. T. & T. was occupied with its rising flood of problems, and it was here that Campbell and Jewett were brought together.

At this time the tide of the Bell System was rising, and Jewett cast himself into it, not to be passively carried along, but to direct it, to open new channels for its increasing volume, to reduce retarding friction, and to remove the obstructions that impeded its flow. While he strove to enhance its power and to increase its usefulness, it, in turn, carried him along with it. They became identified. Jewett was its personification, the embodiment and the representative of the rising tide of research and development that contributed largely to the greatness of the

Bell System, and that in turn made Jewett's greatness evident to all.

In no way and at no time did Jewett claim for himself any of the specific accomplishments that made possible the forward progress. He was always just and generous to his collaborators, giving to them full credit for their work. He pretended to no more than having opened and smoothed their paths. At the presentation of the Edison Medal in 1929, he said: "In accepting this medal, the highest and most cherished honor which American electrical engineers have it in their power to bestow, I do so with a deep feeling that in large measure I am accepting the expression of your appreciation for the work of all the numerous men who have been my intimate associates. On their behalf, as well as my own, I thank you for so signal a recognition of our endeavors."

There was about Jewett nothing of the ruthless drive, the self-seeking, and the disregard of others that so often is associated with a successful career. His intellect was cool, clear, and penetrating, never impassioned or combative. An infection in his early life had cost him the use of one eye and had seriously impaired the other. All his life he thus endured a handicap that must have been particularly trying to a student. It served to spur rather than to embitter him, however, and he made so little of it that many were never aware of his affliction. To some extent it perhaps partially explains his willingness to learn how things looked to others, which was always characteristic of him.

His character has perhaps best been summarized by one of his most admired friends, Gano Dunn, who at the posthumous presentation of the Hoover Medal said of him:

"As a person there was always about him an air of modesty and quiet distinction, an attitude of immediate and voluntary helpfulness to others wherever he learned of opportunity. His sweet and lovable character made him welcome wherever he went, and bound to him hosts of friends, not only in the fields of science and engineering, but in every walk of life. Disinterested and liberal, of acute intelligence and sensibility to humor, companionship with him was richly rewarded. The soul of

honor in every commitment, to do business with him was a satisfaction and to be his friend an invaluable privilege."

KEY TO ABBREVIATIONS

- Amer. Acad. Arts Sci. Proc. = American Academy of Arts and Sciences, Proceedings
 Amer. Bar Assoc. Jour. = American Bar Association Journal
 A.I.E.E. Jour. = American Institute of Electrical Engineers, Journal
 A.I.E.E. Proc. = American Institute of Electrical Engineers, Proceedings
 A.I.E.E. Trans. = American Institute of Electrical Engineers, Transactions
 Amer. Phil. Soc. Proc. = American Philosophical Society, Proceedings
 Amer. Rev. Rev. = American Review of Reviews
 Bell Lab. Rec. = Bell Laboratories Record
 Bell Syst. Tech. Jour. = Bell System Technical Journal
 Bell Tel. News = Bell Telephone News
 Conn. Soc. Civil Engrs. Proc. = Connecticut Society of Civil Engineers, Proceedings
 Elect. Comm. = Electrical Communication
 Elect. Eng. = Electrical Engineering
 Elect. Jour. = Electrical Journal
 Elect. World = Electrical World
 Frank. Inst. Jour. = Franklin Institute Journal
 Jour. Com. = Journal of Commerce
 Jour. Pat. Off. Soc. = Journal of the Patent Office Society
 Phil. Mag. = Philosophical Magazine
 Phys. Rev. = Physical Review
 Pop. Sci. = Popular Science
 Sci. Mo. = Scientific Monthly
 Trans. Amer. Inst. Chem. Eng. = Transactions, American Institute of Chemical Engineers
 West. Soc. Eng. Jour. = Western Society of Engineers, Journal

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E. P. Kohler

ELMER PETER KOHLER

1865-1938

BY JAMES B. CONANT

Elmer Peter Kohler, Sheldon Emory Professor of Organic Chemistry at Harvard, died at the age of 73 on May 24, 1938. Until a few days before his death he had carried the full burden of academic duties with all the vigor of a man twenty years his junior. The friends who were close to him realized how fortunate he was that he could remain an active chemist almost to his dying hour; a career of forty-five years of teaching and research was brought to a close so suddenly that he never knew the pangs that come to those who reluctantly close the laboratory door behind them on the final day.

Kohler was a man who might have said, "For me to live is organic chemistry." Indeed, his words often conveyed this thought; for example, he ushered a visiting European scientist into his private laboratory with the proud declaration, "Here is my own laboratory; here I am the happiest person in the entire world." And this was no chance remark; to understand Kohler one must realize that he continued throughout his life to devote many hours of every week to experimental undertakings carried out with his own hands. Only those who have seen him in his shirt sleeves, crystallizing and recrystallizing his precious materials on his own laboratory bench, have known the man; and only visitors to whom he explained the intricacies of the problem under study with the aid of pen and scratch paper have heard a great teacher at his best. The biography of Elmer Peter Kohler must be primarily the record of a scientist who loved his subject as an art and who delighted most in teaching the art to eager apprentices who themselves aspired to become masters. Indeed, his career is a standing answer to those who proclaim the false dichotomy between research and teaching. One of the keenest observers of the Harvard scene remarked on learning of Kohler's death, "The University has lost its greatest teacher." Yet the list of more than a hundred papers which is appended to this biography is testimony to the unremitting efforts of an investigator who never ceased to be fascinated by the complexities of the

chemistry of carbon compounds.

At the beginning of this century it was the custom in many colleges and universities for the professor of organic chemistry to give the freshman course. Before the impact of physical chemistry had transformed even the elements of general theory, the qualitative approach of the organic chemist seemed suitable for the general student,—indeed for many it was far more digestible than the more mathematical introduction which is now the standard diet. The type of freshman course common in colleges fifty years ago, while totally inadequate as a foundation for twentieth-century chemistry, had certain educational values for the student majoring in other fields. The theoretical material was simple and self-contained; the exhaustive treatment of the arguments by which Cannizzaro and his contemporaries established the atomic theory in the mid-nineteenth century provided at least one example of the way science develops. The everyday applications of chemistry and its relation to industry were stressed, and there was ample time available for excursions into pleasant and interesting byways. At all events, many college generations of subsequent lawyers, writers, and businessmen have provided eloquent testimony of how much they enjoyed freshman chemistry presented in this fashion. And none more so than those who took Chemistry A under Professor Kohler.

One of the strange contradictions of Kohler's character was his success as a lecturer and his lifelong diffidence as a public speaker. His delight as an artist was clear when he presented a brilliant lecture, either to freshmen or to advanced students in organic chemistry, or on very rare occasions to a "journal meeting." His anxiety before every such performance was equally real, but known to only a very few. This apprehension which almost deserves the adjective "pathological" was the reason why Kohler made it a firm rule never to speak outside his lecture room. But explanations were seldom given for a policy that appeared so curious and arbitrary to many fellow-chemists. He rarely if ever attended scientific meetings. All of which gained him the entirely undeserved reputation of being an academic snob. He suffered thereby the fate of more than one shy person whose aloofness is attributed to pride.

One episode in Kohler's freshman teaching deserves to be recorded for all who enjoy the hazards of experimental demonstrations before a class. The main subject of a certain lecture in Chemistry A was explosives. The long lecture table was covered with samples of gunpowder: mammoth pieces used in naval ordnance, open vessels of smaller grains used in army guns, samples of sporting powder. About the middle of the hour Professor Kohler introduced the topic of gunpowder with the following words, "There is a common misapprehension that when gunpowder is ignited it explodes, but actually if lighted in the open it burns with a quiet flame." Thereupon he poured a little black powder onto an asbestos sheet, struck a match and applied the flame. The result was a flash along the entire table, a roar and heavy clouds of smoke; an old-fashioned chain reaction had taken place. A spark had jumped from the demonstration to the nearest sample of powder and started off the entire exhibit. The students rushed for the fire escapes. Kohler, who had ducked down behind the table, emerged and held up his hand to quiet the incipient panic. Through the smoke one could just see him from the back of the hall. As the milling throng quieted down, he said, "I have sent for some more gunpowder." After the laughter and cheers had subsided, he performed the experiment without mishap, and then firmly stated, "You see it does burn with a quiet flame; the class is dismissed!"

There was nothing exhibitionist about Kohler's teaching: he used lecture table demonstrations not for a show but to demonstrate a point. He drove home with relentless logic whatever he undertook to expound; he had no patience with ill-formulated ideas or partial truths. In short, he was a teacher who was at one and the same time both interesting and rigorous. The same basic technique was employed in his course in advanced organic chemistry. For more than twenty-five years he presented the chemistry of the carbon compounds to juniors, seniors and graduate students in Harvard University. His approach was entirely his own. No textbook has ever been written or ever will be that can in any way accomplish what he succeeded in achieving. If ever the lecture system for advanced

students had a justification, it was in Kohler's Chemistry 5. For the systematic presentation of the facts of organic chemistry, he had not the slightest use. He assumed that in the more elementary course the students had at least become acquainted with the various common types of substances and knew something about the simple functional groups. He presented the problems of organic chemistry as he saw them at the time. The connection between his lectures and his research interest was never far removed. Not that large sections of the material covered were not essentially orthodox topics treated in advanced books on organic chemistry, but the presentation was Kohler's own; a problem was always in the foreground. Spurning anything approaching the usual mode of classifying organic chemistry with its dreary listing of such topics as "Seven Ways of Preparing Aldehydes," "Five Characteristic Reactions of Aromatic Primary Amines," he rearranged his material to focus the attention of the class on a series of scientific adventures.

His success was the result of a combination of years of experience and great native skill in the art of speaking to small audiences. His consistent refusal to speak outside his classroom coupled with his annual destruction of his lecture notes kept this native skill from being damaged by those forces which slowly erode the pedagogic talents of so many academic men. Who, however, but a bachelor whose life was centered in his subject could find the time and patience to work over every year a completely new approach to his subject? Kohler kept turning over in his mind for a week or so before it was due the way "this year" he was going to present a certain topic. He would drop a remark or two to a junior colleague that betrayed his preoccupation. And then, if one were bold enough to intrude on the actual preparation of the lecture the day before, one would find Kohler sitting at his small desk in the office making notes on pages torn from the back of old examination blue books. He seemed never to write out even a fraction of the entire lecture. His notes, when one could catch a glimpse of them, seemed to consist of a series of opening sentences of paragraphs—these, and some formulas of the complicated trans-

formations so dear to the heart of the organic chemist.

In retrospect, I think I see that Kohler had unconsciously developed the habit of speaking to those special audiences of his much as some orators have learned to address vast assemblies. By memorizing the opening lines of a paragraph (or, if necessary reading them), I am told, the rest of the argument comes forth with elegance and ease and in a spontaneous fashion. As one of the most finished speakers I have heard once said, "If you have clearly in your mind or available for your eye the key sentences in a speech, the remainder will take care of itself, provided the audience responds." And Kohler's student audience always did respond in an amazing fashion. Formal lectures on such factual material as crams the books of organic chemistry would not seem to offer much opportunity for exciting enthusiasm among college students. But one could summon a host of witnesses to testify to the arresting rhetoric in the small lecture room of a professor too shy to speak in public.

Some men's lives are fascinating to study because of the complexity of their characters; curious patterns of conflicting and inconsistent trains of thought and action are revealed. Over a span of years some men's behavior resembles that of a tornado following an erratic course over a wide area; the creative energy of more than one scientist has boiled out first in one direction and then another. But Kohler's career was of another more consistent type: his mature life was all of a piece. Yet at first sight it appears full of contradictions. A reserved man, almost pathologically shy, yet a brilliant lecturer; a professor who devoted almost all his working hours to his experiments and his students, yet a man whose opinions and judgments were those of no narrow-minded specialist. A man who only a few times in his life undertook administrative responsibility, yet when he did, won the immediate confidence of all concerned by the solidity of his character and the wisdom of his decisions. Those who on first acquaintance were skeptical of the human qualities of the silent man from the laboratory soon were amazed at his insight. As to the independence of his judgment and the firmness of his opinion when once a position was taken, there was never any

question. Those who had known him as a luncheon or dinner companion at the Faculty Club (where he took all his meals over many years) were not surprised; the sagacity of his views (never too frequently expressed) had become proverbial among his colleagues. Likewise, advanced students who sought his advice felt they were dealing with one who knew whereof he talked or else refrained from talking; they were dealing with a relatively rare phenomenon among professors, a person whose instinctive responses seemed to spring from an age-old acquaintance with the realities of human nature. Whoever succeeded in drawing him into conversation on general topics was soon in contact with a strong dose of common sense. The reaction of a man of the fields rather than of the library characterized the overtones (but not the content) of Kohler's discussion of matters political, whether national, local or academic.

The combination of qualities which together made Kohler such a consistent character can be compared perhaps to those of a mountain guide. The comparison first came to my mind when in preparing this memoir I recalled how gladly Kohler always sought the mountains of Canada, Switzerland and the United States and how fondly he described his long walks through mountain country. But the analogy, I believe, rests on something far less superficial. In the first place, a teacher of advanced students in a discipline where mastery of techniques is essential before one can work alone, is in the same position as a guide on ice or rock with the novices strung out behind him on the rope. He leads the climb both in order to get to the summit (and he enjoys it!) and to show how the art of mountaineering can be mastered. In the second place, a mountain guide has his life anchored to his village; a rural economy and a small group of neighbors provide the fixed reference points to which he instinctively refers both in his adventures on the high peaks and in judging the behavior of his clients from distant lands and cities. He knows human nature in a special way because he combines with the shrewdness of a countryman a wide observation of the foibles of those who can and do gladly come to him from afar for instruction in a hazardous sport. Replace the mountains by the laboratory, the climbing

party by the candidates for the Ph.D., and the mountain village by Egypt, Pennsylvania and the analogy is complete.

Kohler never turned his back on the Pennsylvania "Dutch" community in which he was born and bred. In a sense one may say he never left Lehigh County except for lengthy expeditions to the laboratories in Bryn Mawr and later, Harvard. Not that his homeland intruded into the conversation; quite the contrary. It was hardly ever mentioned. But anyone who talked with Kohler long was in contact with a frame of reference only distantly related to either the laboratory or academic Cambridge.

One might almost say Kohler camped out, bivouacked perhaps, in the spot handiest to his laboratory—a few rooms to provide rest and shelter; breakfast, lunch and dinner at the Faculty Club according to an unvarying schedule; billiards for an hour or so with a few cronies, and then back to the climb; that is, the laboratory. The permanent base was evidently not in Cambridge; it was in fact his family home in Egypt. To this he returned almost always unattended at Christmas, Easter, and for a part at least of every summer. His father and mother were long-lived and clearly the journey home continued to be a central feature of Kohler's plans well past middle age. In the congenial surroundings of his home Kohler found the deepest satisfaction in taking an active part in the workings of a very small community based on the land.

The house in which Kohler was born on November 6, 1865 and from which his body was taken to be buried in the near-by cemetery, had been in the family for generations. He was the son of Lewis A. and Elizabeth (Newhardt) Kohler; the lands which were cultivated by his family were acquired before the American Revolution. Jacob Kohler settled in the vicinity of what was later to be known as Egypt in 1728. His descendants were farmers, millers, storekeepers, in the villages and towns which were dotted over the prosperous farming lands of the so-called Pennsylvania Dutch. The people were bilingual. Muhlenberg College in Allentown (a short distance from Egypt), from which Kohler was graduated in 1886, was in the nineteenth century a "Pennsylvania Dutch" college. The adaptation of the old-world German culture to the highly successful farming life of

Pennsylvania had produced by the end of the nineteenth century a very stable amalgam; only the mechanization of the twentieth century could destroy it. It represented no alien streak in the American pattern. Those who accepted it as a matter of course found no difficulty in moving in and out of communities with more diverse sets of mores. But for one who never married and whose center of daily interest was in the world of the adventures of the mind, the pull of this homogeneous community was irresistible. It would seem that Kohler's ultimate values were those derived from the homestead which stood in the hamlet overlooking the fruitful lands the ancestors had so wisely tended.

Lehigh Valley today is not primarily agricultural. Industry, steel and cement long ago moved in. This process which was taking place in Kohler's youth was everywhere bringing to the younger generation in small communities the realization that the United States was a land of vast distances and great possibilities. Those who were looking for adventure would hardly be content to settle down to the life of their forefathers. So Kohler, like many another of his contemporaries, went West. By accident he seems to have landed a job with the Santa Fe railroad and soon became a traveling passenger agent. Concerned with the welfare of those traveling over the rails seeking a new home and fortune, he came to know the vicissitudes of the prospectors. Perhaps he might have become one himself. But instead, he decided to become a practitioner of the science they most required—the assaying of minerals. For the need for an honest assessment of the samples brought in by lone explorers impressed him greatly; examples of chicanery based on spurious analyses must have come dramatically to his attention. For the purpose of learning to analyze ores, he returned East to begin the study of chemistry at the Johns Hopkins University.

Today a young man of twenty-three who had never studied more than one course in chemistry would be bold indeed who decided so late in life to undertake the training required of a professional chemist. But things were simple in the '80's. Kohler entered the Johns Hopkins University and at once enrolled under one of the leading chemists of the United States, Ira Remsen. (In later years he seemed to think his intimate

knowledge of German was chief among the factors which persuaded Remsen to accept a candidate with so little previous exposure to science.) He completed the necessary formal work in short order and soon began work on a research problem under the immediate direction of Remsen. His interest in minerals and analytical chemistry was short-lived; he had found what he wanted in the joy of investigations of organic chemistry.

Kohler received his Ph.D. degree in 1892 and at once started his academic career. The President of Bryn Mawr, Miss M. Carey Thomas, so famous for her skill in spotting promising young instructors, offered him a position as an instructor. For twenty years he taught general and organic chemistry in this famous women's college. In due course he advanced through the academic hierarchy and was made a professor in 1900. Not long after, when the head of the department, Professor Keiser, left to take a position in another educational institution, Kohler became Chairman of the Chemistry Department. In 1912 he was called to Harvard as a professor with the understanding that he would give both the freshman course in general chemistry and the advanced course in organic chemistry.

There are many stories of Kohler's academic independence. One which I had from his own lips about his call to Harvard can now be put on record without embarrassment. Although Kohler in 1912 was forty-seven years of age, had twenty years of academic service and was the author of some thirty papers, it was proposed that he come to Harvard as an assistant professor. (The rate of promotion in many American universities, it must be remembered, was of a different order of magnitude forty years ago from what it is today.) The offer came from the chairman of the department, an eminent chemist, and I am sure was politely tendered. It was equally politely but quite firmly declined. Mutual friends attempted to negotiate as they still sometimes do in academic circles. Kohler refused to budge. Finally, the President of the University, learning of these discussions and having his own independent assessment of Kohler, wrote a few lines asking if the Bryn Mawr professor would come to Harvard as a professor of chemistry. Kohler replied in not more than three lines in the affirmative. The appoint-

ment was made and, as Kohler more than once said privately, he had a "presidential appointment," and was in a sense, he felt, "independent" of the department.

This story must be balanced by a clear statement that though Kohler was certainly a very independent member of the department, he was also cooperative to a high degree and never independent for the sake of asserting any petty rights. He cheerfully carried more than his share of the teaching load, and later in his life was an effective chairman for a long period of time. As a wise and understanding older man he assisted not only his students but more than one junior member of the department in solving human problems. As a shrewd judge of character as well as of scientific ability he helped shape the department policy so that, as far as possible, only the most likely candidates received appointments. In guiding his students and his junior colleagues into academic and industrial positions, he was utterly candid to the prospective employer, yet sympathetic and helpful to the younger man.

Kohler's contribution to science is difficult to summarize in any brief biography. The qualities which made him so excellent an instructor of the novice in research to some degree impaired his own effectiveness. To use an overworked analogy, he was a tactician and not a strategist. With good fortune he might have made an experimental discovery which would have assured his work a place on the permanent roll of the outstanding contributors to organic chemistry. He had no such luck, however. Many of his papers are important, none of them is trivial, but none quite hits the bull's eye.

His mode of work was determined largely by his keen interest in the art of organic chemistry. To return to the analogy of a Swiss guide, he was fascinated by the technique and loved to do what he could with stubborn material with his own hands. By the time he had come to Harvard he was aware of the fact that many complicated transformations of carbon compounds could be followed if one could isolate pure compounds in each step. As the techniques of experimentation then stood, this was possible only if one could obtain crystalline products. By fractional crystallization, particularly in Kohler's skillful hands, many

complicated mixtures could be separated. If one were dealing with liquids, distillation as then practiced would have been a hopeless method of separating pure substances. And I must remind the readers of this biography who lack chemical training that the purity of a substance is to the chemist what the accurate control of a physical variable is to a physicist.

In order to work with crystalline material, Kohler studied the transformation of what are often called "side chain compounds." He was interested in the behavior of compounds whose characteristic structure contained the double carbon link of ethylene ($C=C$) and the carbonyl group of aldehydes and ketones (CO). But a study of his papers or the excellent résumé of this phase of Kohler's work in Chapter VI, Volume I of Gilman's *ORGANIC CHEMISTRY* (1942) demonstrates that in almost every case the structure included one or more aromatic residues (e.g. phenyl groups C_6H_5). The presence of such groups conferred on the compounds the physical properties Kohler desired, namely solubility in organic solvents, crystalline form and melting points between 50° and 200° C. Alas, more often than not, however, a mixture of such substances presented to the unwary investigator only a brittle resin or a gummy material. Kohler and his students spent endless hours coaxing crystals from such unattractive tars. The patience and skill needed for success in this enterprise cannot be portrayed to the uninitiated by any words.

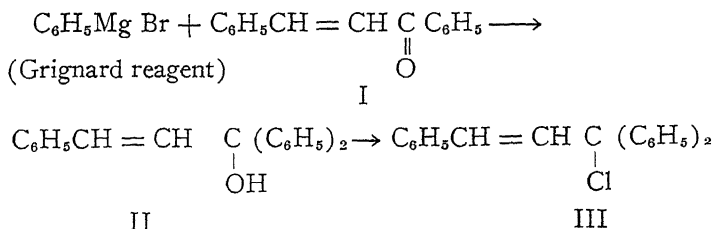
The bulk of Kohler's work dealt with the reaction of various types of compounds of the general nature just described. In the parlance of the craft he was concerned with the behavior of unsaturated and conjugated systems. He had only a very slight interest, however, in the problems that were then arising in industry which were connected with such behavior. This fact is both characteristic of the man and symptomatic of the times. A quick glance at the history of physics and chemistry from, say 1880 to 1930, will show that in this period the academic men for the most part made a virtue of being uninterested in any application of their science. Maxwell's famous and rather snobbish account to a British audience in the 70's of Graham Bell's invention is indicative of the trend. Rowland, addressing

his fellow physicists of America in 1899 said, "He who makes two blades of grass grow where one grew before is the benefactor of mankind; but he who obscurely worked to find the laws of such growth is the intellectual superior as well as the greater benefactor of the two." Some of the academic men on both sides of the Atlantic were in touch with industrial developments occasionally to their financial benefit. But, on the whole, scientists rallied around one banner; inventors, engineers and practical men, around another. Kohler was a strict member of the scientific fraternity. As a consequence, though some of the revolutions in applied chemistry during his lifetime were very close to his own sphere of scientific work (synthetic rubber and plastics, to name but two), Kohler pursued his study of unsaturated compounds without reference to those industries founded on the unique behavior of such substances.

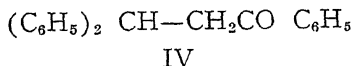
Early in his career as an investigator, Kohler encountered a completely unexpected reaction of a class of compounds known as alpha beta unsaturated ketones. The exploration of the peculiarities thus uncovered, almost by accident, led him into a careful study of the phenomenon known as 1,4 addition. Both the origin of the problem and Kohler's method of completing the research are so characteristic as to warrant the intrusion of some details of organic chemistry into this general account of a chemist's life.

Shortly after the turn of the century, the attention of organic chemists was attracted by two discoveries in widely separate fields. Grignard, a French chemist, showed his fellow workers a new powerful synthetic tool in the form of an ethereal solution of organic magnesium halides, shortly to become known as the Grignard reagent; by means of such compounds as phenyl magnesium bromide C_6H_5MgBr , carbonyl compounds (ketones and aldehydes) could be transformed into carbinols. Moses Gomberg at the University of Michigan had reported some very strange reactions of triphenylmethyl halides when treated with metals and had interpreted his results by assuming the existence of the free radical triphenylmethyl. Kohler as was his practice throughout his life had kept in close touch with the recent publications and realized the full significance of both Grignard's and

Gomberg's work. He thought he saw the possibility of using the Grignard reagent to prepare a chloride similar to triphenylmethyl chloride but differing from it by the presence of an ethylenic linkage in its structure. He envisioned the following reaction:

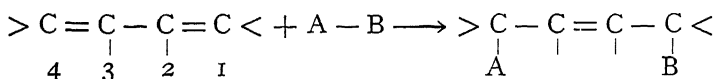


A comparison of the last formula written (III) with that of triphenylmethyl chloride, $(\text{C}_6\text{H}_5)_3\text{C Cl}$, makes evident the analogy Kohler had in mind. Would the group $\text{C}_6\text{H}_5\text{CH}=\text{CH}-$ in combination with only *two* phenyl groups confer the peculiar properties on the halide which Gomberg had reported? Kohler thought the unsaturated linkage $\text{CH}=\text{CH}-$ would be roughly the equivalent of the phenyl group, and subsequent work several decades later proved his intuition to have been correct. But Kohler was not to explore these possibilities; for it soon developed that the reaction he had envisaged proceeded quite differently from what had been confidently predicted. The interaction of phenyl magnesium bromide and benzalacetophenone (I) yielded a compound whose structure was that of a saturated ketone (IV below) not the unsaturated carbonyl (II).

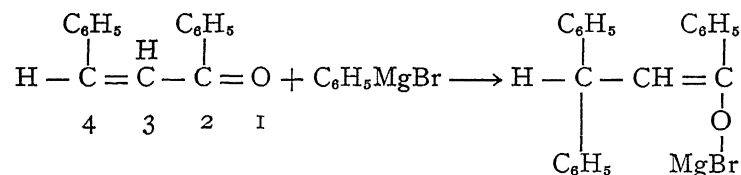


Why had things taken this unpredicted course? This was the type of question Kohler loved and illustrates the kind of chemical research to which he devoted almost all his labors. There were really two questions: (1) By what steps had the transformation taken place, *i.e.*, what was the mechanism of the reaction? (2) What were the peculiarities of the structure of benzalacetophenone (I) that resulted in the Grignard reagent apparently uniting with the ethylenic linkage ($\text{C}=\text{C}$) rather

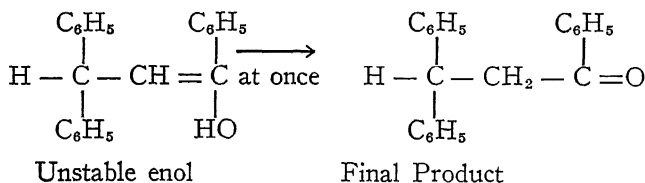
than the carbonyl group ($C=O$)? After Kohler had become convinced of the true structure of his product, he saw that the reaction he had been studying was similar to those made famous by the German chemist Thiele at just about this time. Thiele had directed attention to the anomalous behavior of certain compounds containing ethylenic linkages in alternation (christened by him "conjugated linkages") and elaborated a theory of "partial valence" to account for the fact that addition reactions of such systems could proceed as follows:



As to the theory, Kohler was indifferent; but to the significance of the fact of 1,4 addition, he was very much alive. He proceeded to demonstrate to the satisfaction of most critics that the first step in the reaction between phenyl magnesium bromide and benzalacetophenone was an example of 1,4 addition to a conjugated system followed by the shift of an enolic hydrogen atom after the reaction mixture was acidified. The mechanism was thus:



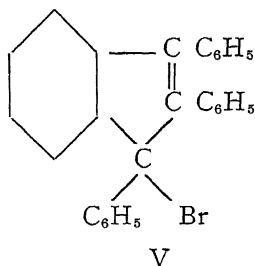
on treating with acid



The nature of his evidence for the structure of the first product was not all that might be desired and he later returned to the

$\text{CH}(\text{CH}_3)_2$ or $\text{C}(\text{CH}_3)_3$, 1,4 addition predominates if A is either C_6H_5 or C_2H_5 and is appreciably less in the first case than the second. In short, Kohler demonstrated that the way a conjugated system may react depends both on the substituents and the character of the addendum. All the work in this field which has been done since 1907, it is fair to say, has added relatively little to these principles.

Kohler returned from time to time to a study of conjugated systems always employing the same technique of using phenyl groups or substituted phenyl groups to insure the desired physical characteristics of the compounds. In a series of papers in the 1930's he amplified the data on the effect of substituents on the mode of addition of unsaturated ketones. It is interesting and significant that he did not pursue his original intention of studying the interaction of unsaturated halides and metals except for one observation. In a paper published in 1908 he showed that a combination of phenyl groups and unsaturation in a ring system conferred upon an adjacent carbon-halogen bond some of the properties characteristic of triphenylmethyl chloride. For on treating 1,2,3, triphenylindyl bromide (V) with metals he obtained a solution



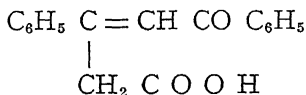
which showed all the properties characteristic of that of a free radical. The point he set out to test when he started on the abortive synthesis of the unsaturated carbinol (II) might be considered to have been settled. Yet clearly there was a great deal more work to do if one were to determine what other group could be substituted for the phenyl groups of triphenylmethyl. Indeed in 1908 it was far from clear that

triphenylmethyl was the dissociation product of an "orthodox" crystalline material hexaphenylethane. Other chemists took up the lead given them by Gomberg's highly significant publication. But Kohler often said when discussing the history of this subject that he had no desire to rush into another man's territory and exploit someone else's discovery.

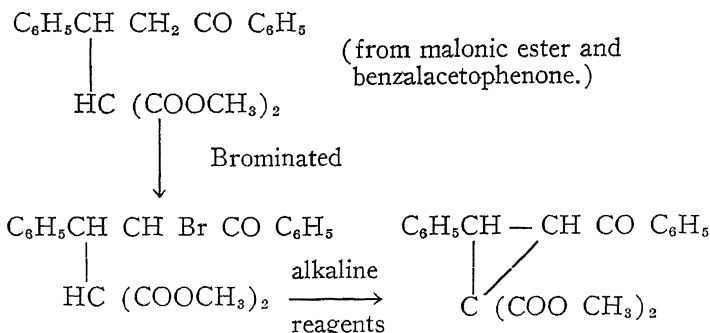
At all events, in the second decade of the nineteenth century Kohler turned his attention to the three carbon system characteristic of glutaconic acid of which the formula is:



The unsymmetrical derivatives of this acid can exist in several isomeric forms depending on the structural and stereochemical relations of the substituents to the double bond. In the last forty years all the phenomena have been adequately accounted for without postulating any special type of linkage. But in the period 1910-1920, there was discussion of the possibility that the system—CH₂ — CH = CH—should be formulated—CH — $\begin{array}{c} \text{[H]} \\ | \end{array}$ CH — CH to indicate the lability of the hydrogen atom, as some investigators were then inclined when discussing the behavior of acetoacetic ester. Kohler felt that labile isomers could be studied only if crystalline solids were at hand. Therefore, he undertook to prepare compounds of the following type:

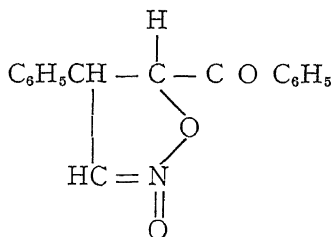


He started with malonic ester and his old friend benzalacetophenone. But once again the vagaries of organic reactions led him into an unsuspected field. The desired unsaturated keto acid was not obtained; instead a convenient method of preparing keto-cyclopropanes was discovered. The steps in the transformation were shown to be as follows:



Having been thus brought to the study of the simplest of alicyclic systems, Kohler and his graduate students at Harvard devoted considerable attention to the reactions of substituted cyclopropanes. They showed that the labile ring in keto cyclopropane acids could open in several ways, and the reactions were to some degree reminiscent of alpha beta unsaturated ketones.

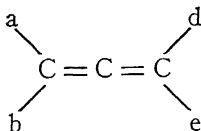
Nitro compounds had long been a matter of interest to Kohler; therefore, it was not surprising that he should seek to discover the behavior of nitrocyclopropanes which he showed could be readily prepared by a series of reactions similar to those indicated above if nitromethane were substituted for malonic ester. But there was also formed in the course of these reactions a type of compound known as isoxazoline oxides of which the following formula may serve as an example:



Here was a convenient road into a new and fascinating type of unsaturated compound. By the use of appropriate aryl groups crystalline products could be obtained (at least by Kohler though the technical difficulties were often great). A

study of the behavior of compounds containing the $-\text{C}=\overset{|}{\text{N}}=\text{O}$ system led into a consideration of other unsaturated nitrogen compounds. Then followed papers on pseudo bases, cyclic nitrones as well as further studies on isoxazoline oxides.

From first to last Kohler was interested in theoretical organic chemistry. With all the developments in this wide field he was in close touch and as the preceding paragraphs attempt to show, he made significant contributions to the study of the mechanism of a wide variety of reactions. He had little interest in the classic examples of the elucidation of the structure of complex plant and animal compounds. In his course he devoted little or no time to those investigations of the alkaloids, the terpenes, the natural dyes and the polypeptides which were generally accounted among the triumphs of nineteenth century organic chemistry. Sugar chemistry was another story: this he expounded with elegance and enthusiasm, probably both because it illustrates so well the principles of stereochemistry and also because of the obvious connection with medicine and biology. In the last years of his work he made one of his most striking contributions by his resolution of an allenic compound into the optical isomers (with J. T. Walker and M. Tishler 1935). By so doing he brought further experimental evidence to show that the phenomenon of optical activity is connected with an asymmetric *molecule*. Stereochemical models of allenic compounds of the following type show that two mirror images are possible; this had been well known as a theoretical possibility but it remained for Kohler and his students to prepare a compound with such groups as permitted resolution by the usual methods of stereochemistry.



Allenic Compound having two optically active isomers

One may conclude this memoir by a word as to Kohler's role in World War I. He was brought to Washington as one of

the two top civilian advisers to those in charge of the research work of the Chemical Warfare Service. The area assigned to him was offense, while his opposite number, Professor W. K. Lewis of the Massachusetts Institute of Technology, was responsible for the defensive work. In this one and only excursion into administrative work away from the academic scene, Kohler showed his wisdom about human problems as well as his insight into scientific matters. Though the term of service was short, all who came in contact with him were deeply impressed by the effectiveness of his labors. In the subsequent years there were many occasions when his assistance would have been gladly welcomed by those concerned with what might be called the public aspects of science. But Kohler was not interested. He would leave his laboratory though reluctantly, to perform a patriotic duty in a World War. But no less an emergency could justify his parting from his beloved compounds and their vagaries.

Kohler was elected to the National Academy of Sciences (Section of Chemistry) in 1920. His place in the history of American chemistry is assured by his publications and above all by his influence on his students. To all who had the rare privilege of working closely with him, his unique personality will remain one of the compelling forces in their lives.

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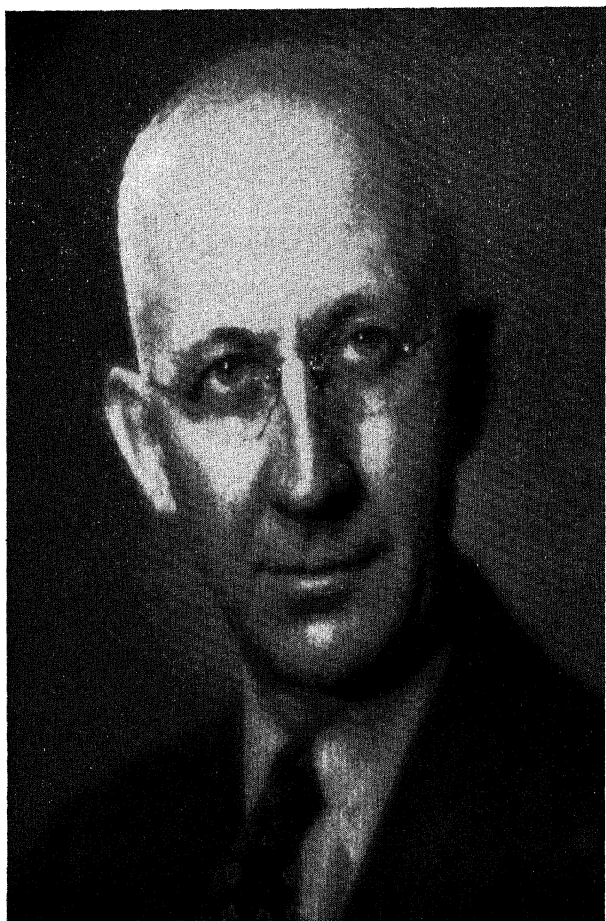
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Homer Atkins

HOMER BURTON ADKINS

1892-1949

BY FARRINGTON DANIELS

Organic chemistry in the United States underwent an extraordinary expansion in the three decades which included the two world wars. This period of development was characterized by a rapid increase of laboratory knowledge which was quickly applied to industry; and also by a large expansion in the number of organic chemists who had graduate training in research. Among the pioneering and influential leaders of this period was Homer Adkins, whose death on August 10, 1949, at the age of fifty-seven came as a shock to his many friends. He was the world's authority on hydrogenation of organic compounds; he made important laboratory contributions to the mechanisms of catalyzed reactions of organic chemistry and he led the teaching of organic chemistry at the University of Wisconsin for thirty years, during which time he trained over one hundred Ph.D. students who are now in positions of responsibility and leadership in industry and education. He belonged to the close-knit group of leaders including Adams, Conant, Gilman, Marvel, Whitmore and others who did much to influence the course of organic chemistry throughout the second quarter of the twentieth century.

Born January 16, 1892, in Newport, Washington County, Ohio, the son of Alvin and Emily Middleswart Adkins, Homer Adkins was brought up with a brother and a sister on a peaceful farm located in a bend of the Ohio River. He attended high school at Newport and entered Denison University as a tall, shy, earnest country boy. He graduated in 1915 in three and a half years. He then spent three years in the graduate school at Ohio State University, taking his Master's degree in 1916 and his Ph.D. under the direction of Professor William Lloyd Evans in 1918. Soon after receiving his degree in early 1918, he began work as a research chemist for the War Department. The following academic year he spent as instructor in organic chemistry at Ohio State University and in the summer of 1919 he was a research chemist with E. I. Du Pont De Nemours and Company.

In 1919 he came to the University of Wisconsin, where he remained continuously until his death thirty years later, except for two summers of work in industry at the Bakelite Corporation in 1924 and 1926 and for responsibilities from 1942 to 1945 as administrator and research director in the war program of the National Defense Research Committee and the Office of Scientific Research and Development. He never took a leave of absence from the University of Wisconsin and was, in fact, on the teaching or research staff continuously, including nearly every summer. He was an indefatigable worker who devoted his life unstintingly to the teaching of organic chemistry, to the affairs of the University, to the advancement of organic chemistry through research, and to the application of organic chemistry to American industry and the war effort.

As a teacher, Adkins was precise, lucid and interesting. His books and published papers are models of clear, scientific expression. Throughout his whole career at the University of Wisconsin, he lectured to graduate students in a course called "Survey of Organic Chemistry" but he also kept contact with elementary students and continued for most of the time to give lectures in the first course in organic chemistry. His lectures were lively and interesting. They were witty and critical, sometimes to the point of being caustic. His point of view, recalled by one of his graduate students, is indicated by a statement made in a lecture in which he said that when he was a young man starting his career and casting about for a field of research, he decided that the field of catalytic reactions was a good one because there was a large body of facts waiting to be correlated. If someone were to examine all of these facts, he would surely arrive at a general theory of catalysis which would then cover all catalytic reactions and lead to successful predictions in the choice of catalysts. But, he commented, after studying catalytic hydrogenation and other catalytic reactions for a quarter of a century his present conclusion was, "What we need is more facts."

Professor Adkins used simple and effective illustrations and analogies in his teaching. He was among the first to emphasize the importance of distinguishing between the rates of organic

reactions and the extent of such reactions at equilibrium. He presented this concept to his class with a simple and obvious illustration: One could not predict the destination of two automobiles going west at equal speeds past the Chemistry Building; the destination of one might be Shorewood (a western suburb) while the other might be headed for Minneapolis.

Not only as a lecturer and teacher was Professor Adkins successful, but also as a writer of books. His chief research contributions in the field of hydrogenation are summarized in the book "Reactions of Hydrogen" published by the University of Wisconsin Press in 1937. He was also co-author of "Practice of Organic Chemistry" and "Elementary Organic Chemistry" both published by the McGraw-Hill Book Company. He wrote the chapter on "Comparison of Chemical Reactivity" in Gilman's treatise on "Organic Chemistry" and with R. L. Shriner another chapter in the second edition on "Catalytic Hydrogenation and Hydrogenolysis." In addition to these books and chapters which have been read by thousands of organic chemists in training, Homer Adkins was also a member of the Board of Editors of Gilman's "Organic Chemistry" and a member of the Board of Editors of "Organic Syntheses" published by John Wiley and Sons. He became an editor of Adams' "Organic Reactions" less than a year before he died. To this publication he contributed a chapter on "The Catalytic Reductions of Esters" which will appear in Vol. VII.

Homer Adkins never lost personal contact with the laboratory. Even through his last years, he spent an appreciable amount of his time with his coat off working with his own hands in the laboratory adjacent to his office. He designed precision equipment for carrying out hydrogenation reactions under high pressures. In this development of high pressure equipment, Professor Adkins was aided by Mr. Lee Henke of the Chemistry Department Machine Shop, and the equipment was later developed and made available to all laboratories through the American Precision Instrument Company.

Adkins' approach to his research laid great emphasis on experiments with a minimum of theory and of mathematics. He expressed himself once as considering it unethical to speculate

very far beyond his laboratory data. He was a master of expression and could describe with great clarity the findings of his laboratory investigations.

He maintained a close contact with industry and was thereby able to bring to his graduate students, to his classes and to his research a fresh, practical and vigorous enthusiasm. He was a consultant to the Rohm and Haas Company from 1932 until his death and to Merck and Company from 1940 until his death. In 1944 he was a consultant to Charles Pfizer and Company and in 1946 to the General Aniline and Film Corporation. He never allowed these contacts to interfere with his research or his teaching, but he acted as an effective and important liaison professor, strengthening and accelerating the fundamental developments in industry, while at the same time adding to his ability to stimulate his students.

As an administrator, Adkins carried heavy responsibilities throughout the last war. From September 1940 to April 1946 he was Official Investigator in charge of a number of research contracts between the University of Wisconsin and the Office of Scientific Research and Development. From May to December 1942 he was in charge of Section B-3C of the National Defense Research Committee which was charged with research on "Protective Ointments and Fabrics." From December 1941 to February 1943 he was in charge of the Organic Chemical Group at the NDRC Explosive Laboratory at Bruceton, Pennsylvania. From January 1943 to December 1945, he was a member of Division 9 of the Office of Scientific Research and Development, in charge of Section one. From October 1940 to February 28, 1946, the National Defense Research Committee and the Committee on Medical Research assigned numerous investigations to be carried out at the University of Wisconsin by Professor Adkins and his assistants under a series of contracts (NDCrc-6, OEMsr-78, OEMsr-304 and OEMcmr 567). The problems involved in these investigations for chemical warfare involved synthesis of various chemical reagents, protective measures for clothing, problems of detoxification and decontamination and evaluation of effectiveness of agents against mustard gas and a

determination of the storage stability of various protective agents.

Professor Adkins was active in the affairs of the Chemistry Department of the University of Wisconsin and served on many committees. He was one of the vigorous leaders in departmental policy making. Professor J. H. Mathews, who brought Professor Adkins to Wisconsin and remained chairman of the department throughout the whole period of Adkins' life at Wisconsin, aided vigorously in obtaining equipment and support for Professor Adkins' work. Many of his publications acknowledge the help given to his research program by the Research Committee of the Graduate School and the Wisconsin Alumni Research Foundation.

Vigorous, intense, outspoken, not always consistent, a trial at times to his departmental chairman and his associates, Homer Adkins fought hard nevertheless for democracy and democratic procedures as he saw them. Intolerant of inefficiency and evasiveness, "He loved to present a good argument and to be opposed by one." He espoused vigorously the cause of anyone whom he thought to be the victim of injustice. In social groups and faculty gatherings, he was interesting, witty and often brilliant. He loved to make startling statements, but on the whole he could be classed as a moderate democrat.

Adkins served on several University committees, important among which was the Committee on the Evaluation of the Experimental College. He also served on a committee which developed and inaugurated the Program of Integrated Liberal Studies of the College of Letters and Science at the University of Wisconsin. He never shrank from his responsibilities to his department, to his University, to his chemical organizations, or to his nation.

He was an excellent judge of men and was responsible for selecting his outstanding associates, S. M. McElvain, W. S. Johnson and A. L. Wilds, who carry on the responsibilities for organic chemistry which he built up so effectively at the University of Wisconsin.

Professor Adkins was active in the affairs of the American Chemical Society and particularly the Division of Organic

Chemistry. He was Chairman of this Division in 1932 and served many times as a member of its Executive Committee. At the time of his death he had been invited to be a nominee for the presidency of the American Chemical Society. He served also as Chairman of the Wisconsin Section of the American Chemical Society and was elected many different times to serve as Councilor from the Wisconsin Section.

Homer Adkins was married on February 21, 1917, while he was a graduate student at the Ohio State University, to Louise Spivey, who had been a classmate at Denison and who was teaching high school mathematics. There are three children: Susanne Dorothea (Mrs. Gordon Chadek) who has three children; Nance, who served with the Marine Corps during the war; and Roger, who has enlisted in the U. S. Army after a short time at the University of Virginia.

Professor Adkins devoted most of his time to his professional life. He did find time, however, to read many books and his particular outside field of study and interest was the history of the Civil War. His reading was enlivened by several visits to the battlegrounds of that war. He took up golf enthusiastically for in it he found relaxation and healthful exercise. He was a charter member of the Blackhawk Country Club. He was a member of the First Congregational Church and served as deacon from 1926 to 1928.

Homer Adkins' death was undoubtedly hastened by the heavy responsibilities and extended travel which were occasioned by the war. Carrying a program of teaching, maintaining a large research program in his laboratories and the directing and administering of many researches in the east, combined with the war time pressure, took a heavy toll of his strength. In the late spring of 1949, he apparently had a slight attack while playing golf and concluded that as soon as he could get time he would have a complete medical checkup. The organic Symposium was held in Madison in June, 1949 and was attended by nearly one thousand organic chemists. Homer Adkins put much energy and time into the plans and organization for this Symposium. In the late afternoon of June 20, he presented a paper describing some of his recent research to

the Symposium. After this meeting he took a group of interested chemists to his laboratory to inspect some of his special high pressure equipment. On his way back to the meeting headquarters, he was stricken with a heart attack and was taken to the hospital where he stayed for a month or so. Later on he returned home and seemed to be improving, but quite suddenly began to fail rapidly and died on August 10, 1949.

Characteristic of his strong spirit and humor was a mimeographed letter which he sent to all of his associates and friends on July 5, 1949, which read as follows:

On Monday evening, June twentieth, I went out of circulation on account of a coronary occlusion, which resulted in some damage to my heart. Since then I have been very comfortably located at the Madison General Hospital and am recovering. Probably I will be here until about the middle of July, after which I expect to be at home.

I am sorry to have missed and inconvenienced so many of my friends. Practically speaking, I have no activities and it appears that I will be very limited in what I will be allowed to do for the next month or so. I feel better than I have for a long time, in fact I recommend my current mode of vacation very highly. I expect to be carrying on my normal activities after the prescribed rest at home.

Homer Adkins received many honors. He received the honorary degree, Doctor of Science, from Denison University, his alma mater, in 1938. He was awarded the "Medal for Merit" by President Truman for conduct and administration of investigations under the Office of Scientific Research and Development 1941-1945. He was elected member of the National Academy of Sciences in 1942. He was a member of Phi Beta Kappa, Sigma Xi, Phi Delta Theta, Alpha Chi Sigma and Phi Lambda Upsilon. He was a Fellow of the Chemical Society of London, a member of the Swiss Chemical Society, a member of the American Association of University Professors, a member of the Chemists Club of New York.

After his death, Adkins' associates and many former students and friends, under the leadership of Dr. Ralph Connor and Dr. S. M. McElvain, raised a fund in his honor and for the next few years the Homer Adkins Fellowship will support a

graduate student in chemistry at the University of Wisconsin. The first recipient of this fellowship has been chosen. The Chemistry Department gratefully remembers the contributions and the leadership of Homer Adkins and considers the fellowship named for him to be the highest honor that it can bestow on an outstanding graduate student.

President Edwin B. Fred of the University of Wisconsin said, "He was recognized as one of the leading chemists that America has produced. He was the kind of man who makes a University distinguished," and President James B. Conant of Harvard said, "the academic world has suffered an irreparable loss."

HOMER ADKINS' CONTRIBUTIONS TO ORGANIC CHEMISTRY

In American Men of Science, Adkins listed his research interests as "causation of organic chemical reactions; relations of catalysis, reactivity and structure of organic compounds; the reaction of hydrogen with organic compounds; oxidation potential." He was always hoping for generalizations which would correlate the many facts of organic chemistry but in practice he could not bring himself to go very far away from the laboratory and empirical approach. From his laboratory have come a large number of important contributions dealing with hydrogenation and with factual material on chemical equilibrium, rates of reaction, and molecular structure which have given us a better understanding of the principles of chemical reactivity in organic chemistry. The principles and techniques which Adkins and his students developed have been important in synthetic organic chemistry both in the laboratory and in industry.

A complete bibliography with descriptive titles is given at the end of this biography which shows clearly the range of Professor Adkins' interests and the influence of his researches on the development of organic chemistry. Comments can be made on only a few fields of activity selected from this record in chronological order.

Homer Adkins' first publication in 1919 was based on his Ph.D. thesis under the direction of Professor William Lloyd Evans at the Ohio State University. All the rest of his work was done at the University of Wisconsin and most all of it has been published in the *Journal of the American Chemical Society*.

His Doctor's thesis was concerned with the rates of oxidation by potassium permanganate of acetaldehyde, oxalic acid and other organic materials and the influence of the different variables such as temperature on these rates. The nature of the intermediates and the influence of molecular structure were the points of his chief interest.

His second paper was based on an attempt to synthesize a vat dye from phenanthrene, perhaps on outgrowth of interests acquired during an earlier summer's work in the Du Pont laboratories.

His third research involved the catalytic action of oxides on esters and profoundly affected the whole course of his research interests. Bancroft had pointed out the widely different nature of the products obtained from a given starting material depending on whether the catalyst was an oxide of aluminum, titanium or thorium. Adkins verified the marked specific action of the different catalysts and hoped that it would be "better to study the types of reactions as related to the simple surface conditions of the solid catalyst rather than to the complex phenomena which occur in liquid solutions."

Further studies of the catalysts led to new methods of preparing catalysts by heating aluminum alkoxides to give aluminum oxides with different spacings depending on the alkyl groups involved.

Acetal was used by Professor Adkins in several different researches in his studies of chemical reactivity and catalysis. This work led to studies of hemi-acetal.

The study of different catalysts soon led to the use of promoters in oxide catalysts and to mixed oxides. A copper-chromite catalyst was one of Dr. Adkins' chief contributions and with it he was able to hydrogenate esters to alcohols. He always retained a special interest in this catalyst and one of his last papers was concerned with its structure and catalytic

mechanism. Adkins next took up metallic catalysts and made a great deal of use of the Raney nickel catalyst.

In extending this hydrogenation to many compounds, Adkins saw the need for going to higher and higher pressures and much of his experimental ingenuity was devoted to designing practical equipment with thick walled bombs for carrying out this work. He devoted most all of his attention to reactions in which the catalyst was suspended in the liquid phase and the hydrogen under very high pressure was dissolved in the liquid.

After developing the techniques for hydrogenation of unsaturated compounds in which hydrogen is added to a double bond, the next step was to extend the work to more general reactions in which hydrogen reacted on a catalytic surface with a given molecule to split off two new molecules. Adkins gave the name hydrogenolysis to these reactions. He then extended the reactions of hydrogen to an ever widening group of organic compounds, such as nitrogen compounds and ring structures.

In 1937 Dr. Adkins brought all of his work on hydrogenation together in his most important single contribution, the book "Reactions of Hydrogen with Organic Compounds over Copper-Chromium Oxide and Nickel Catalysts." This proved to be a best seller for the newly created University of Wisconsin Press. As evidence of its popularity an order for thirty copies soon came from the Japanese Government. Of his book, Adkins writes in the preface "The present volume is an attempt to correlate and summarize, and indicate the significance of experimental results in the Wisconsin laboratory in the development of high pressure hydrogenation as a tool for use in synthetic organic chemistry.

"I believe that we have considerably extended knowledge with respect to the relationship of the structure of organic compounds to their behavior with hydrogen, and that we have made some progress in the development of methods for selective or preferential hydrogenation and hydrogenolysis. I hope we have described useful and practical apparatus for the reaction of

hydrogen at higher temperatures and pressures. All the observed facts are in harmony with the conception that the catalyst in hydrogenation combines with both the hydrogen acceptor and with hydrogen and each is activated and held in such a position with respect to the other that reaction may ensue."

A later summary of hydrogenation and hydrogenolysis was collected by Adkins and Shriner in Gilman's 2nd edition of "Organic Chemistry" in 1943. In this chapter, new techniques and equipment are described and special instructions given for making catalysts, building high pressure equipment and selecting suitable solvents. Many pages of hydrogen reactions are given with yields to be expected under a variety of conditions.

At the time of his death, Professor Adkins was working on an extensive revision of his book.

Homer Adkins has given to chemistry another classic contribution which brings together a wide range of phenomena. In 1932 he published a paper on the "Comparison of Chemical Reactivity" and in 1938 and 1943 he wrote a chapter in Gilman's "Organic Chemistry" on the same subject. In these writings he clearly distinguishes between equilibrium and reaction rate and points out that different workers have used widely different criteria for comparing chemical reactivity of different chemical compounds and that it is necessary to specify what experimental method is being used as a reference frame. The comparison can be made "by measuring (1) the extent of a reversible reaction, (2) the rates under identical conditions if the reaction is free of side reactions, (3) the severity of conditions (such as temperature) which are necessary to induce a given type of reaction to occur, (4) the relative rates of competitive reactions as measured by the ratio of the products."

From 1940 to 1946 Homer Adkins' research activities were largely taken up with many problems of chemical warfare, as listed later in this biography.

After the war, Professor Adkins returned to his former researches on catalyst behavior and hydrogenation and made a logical extension to carbon monoxide under pressure instead of hydrogen. With these techniques, he was able to convert

alcohols into acids with one additional carbon atom and thus accomplish carbonylation on a practical basis. In his last research, he was interested in working out the best conditions for carrying out the reactions in standard laboratory equipment and determining the identity of the product formed, the nature of the alcohols that can be carbonylated and the effect of structure.

As is the case with most leaders of active academic research, Homer Adkins was influenced by new developments and by the scientific work of his friends. Although his chief effort was directed along his major research, he undertook, nevertheless, several unrelated projects such as the nitration of organic compounds with nitrogen pentoxide, the synthesis of derivatives of ergosterol and cholesterol to be tested for vitamin D potency, and the synthesis of a material which might be related to penicillin. He was quick to appreciate and use new techniques such as oxidation potentials, and the dropping mercury electrode for determining the concentration of organic compounds and calculating equilibrium constants.

Summarizing, Homer Adkins made outstanding contributions to organic chemistry in at least six special fields.

(1) Throughout his whole research program, he sought to predict equilibrium conditions from molecular structure and he distinguished clearly between equilibria and rates of the competing reactions which are striving to reach equilibrium. This work was concerned chiefly with esters, alcohols and acetals, and, to a lesser extent, with unsaturated compounds and nitrogen containing compounds.

(2) Adkins is best known for his catalytic hydrogenation and hydrogenolysis. His studies have given us practical methods of preparation of many types of compounds hitherto obtainable only with great difficulty. These studies have led not only to successful laboratory methods but in addition have proved to be of considerable industrial significance. As examples may be cited the hydrogenation of pyridine and its derivatives and of furfural. Probably the most significant contribution in this field was the catalytic hydrogenation of esters to give primary alcohols over a copper-chromium oxide catalyst. This

procedure has found wide industrial application in the preparation of the higher alcohols from the naturally occurring esters of the fatty acids, as for example in the vegetable oils. The availability of these higher alcohols from this hydrogenation process has made possible the development of the important group of detergents known as the sodium alkyl sulfates.

(3) Adkins' experimental techniques have been made available for making catalysts and for using them for hydrogenation under high pressures in special bombs. They are used widely by chemists all over the world.

(4) The studies on hydrogenation over metal catalysts led naturally to the development of methods by which organic compounds can be dehydrogenated by the same catalysts in the presence of certain hydrogen acceptors such as benzene. This dehydrogenation process marked quite a step forward in the preparation of certain organic compounds by the removal of hydrogen from cheaper raw materials.

(5) In hundreds of reactions, Homer Adkins determined and recorded equilibrium constants or oxidation potentials, thus providing data which can be used in present and future attempts to predict chemical reactivity from molecular structure.

(6) An expansion after the war of the high pressure catalytic techniques led to the addition of carbon monoxide to alcohols in order to convert them into acids.

It is safe to predict that if his career had not been ended prematurely, he would have extended his techniques and his background of research to valuable new methods of synthesis and to a still wider understanding of organic chemical reactions.

A LIST OF THE INVESTIGATIONS AND REPORTS BY HOMER ADKINS UNDER CONTRACTS BE- TWEEN THE OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT AND THE UNIVERSITY OF WISCONSIN

The National Defense Research Committee and the Committee on Medical Research, during the period from 1 October

1940 to 28 February 1946, assigned numerous investigations to be carried out at the University of Wisconsin under a series of contracts, NDCrc-6, OEMsr-78, OEMsr-304 and OEMcmr-567. The problems studied in the laboratories in Madison were as follows:

1. Synthesis of compounds of possible value in chemical warfare as poisons, vesicants, lachrymators, or sternutators.
2. Synthesis and development of methods for the preparation on a large scale of compounds for use in ointments or clothing, for protection against vesicants and toxic agents.
3. Synthesis of compounds for the detection of chemical warfare agents.
4. Synthesis of compounds requested by representatives of the Army or Navy, for purposes not disclosed.
5. Studies on detoxification and decontamination of chemical warfare agents.
6. Evaluation of chloroamides for relative effectiveness against mustard and other vesicants.
7. Determination of thermal, hydrolytic, and storage stability of chloroamides in the pure state, on clothing, and in ointments.
8. Development and application of methods for evaluation of impregnated or coated fabrics protective against mustard and other vesicants.
9. Evaluation of a method for the preparation of hydrogen peroxide.
10. Formulation of protective ointments.
11. Evaluation of the irritancy of various chloroamides when applied to human skin in various ointment formulations.

12. Development of methods for the effective utilization of non-volatile poisons in warfare.

13. Synthesis of potential antimalarial agents and intermediates.

Thirty-eight formal OSRD reports on projects listed above (restricted).

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KEY TO ABBREVIATIONS

Ind. Eng. Chem. = Industrial and Engineering Chemistry

J.A.C.S. = Journal of the American Chemical Society

J. Chem. Educ. = Journal of Chemical Education

J. Phys. Chem. = Journal of Physical Chemistry

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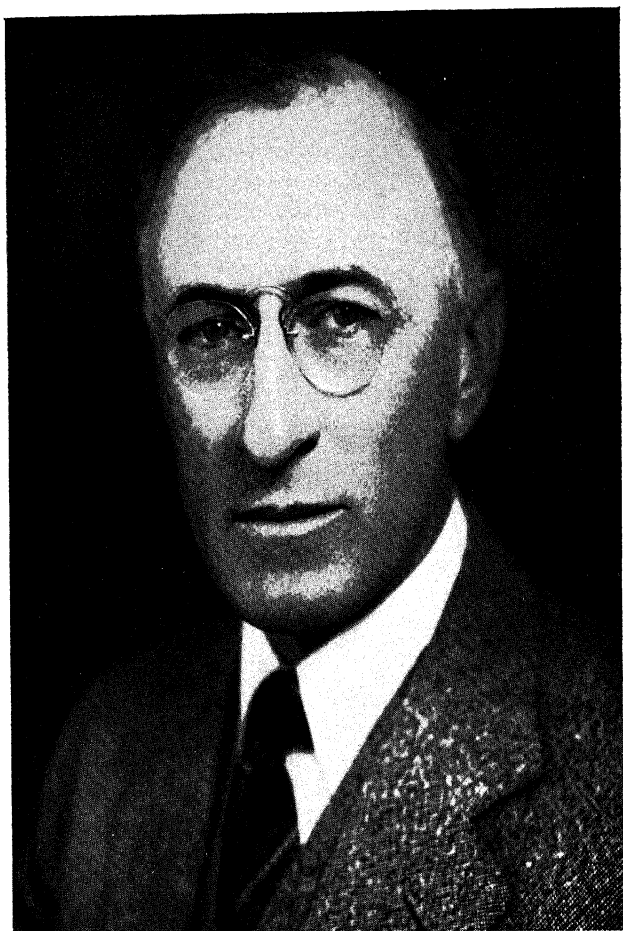
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A. J. Dempster.

ARTHUR JEFFREY DEMPSTER*

1886-1950

BY SAMUEL KING ALLISON

Arthur Jeffrey Dempster, one of the outstanding physicists of our time and one of the most eminent of Canadian-born scientists, was born to a prosperous Scotch-Irish family in Toronto on August 14, 1886. His parents were James and Emily (Cheney) Dempster. His scientific career extended from 1911 (the date of his first paper) to almost the day of his death, March 11, 1950. It may be called almost a linear career; for he devoted himself almost exclusively to a single task, which was the discovery of stable isotopes of the chemical elements and the measurement of their relative abundances. He discovered a greater number of stable isotopes than anyone else except the man who opened this field of research, F. W. Aston. This is a distinction which his name is almost certain to retain for all time.

A yellowed page of a Toronto daily newspaper of 1904, preserved by his family, shows that in his teens he won either first or second rank in competitions for scholarships in almost every field of human knowledge. This is the first published evidence of his extraordinary breadth of interest, which was to remain one of his characteristics till the end. He attended the University of Toronto, obtaining the A.B. in 1909 and the M.A. in 1910. His earliest scientific publication seems to be a discussion of Darwin's tidal theory published in the *Journal of the Royal Astronomical Society of Canada* in 1911. This brief venture into geophysics was not repeated, and in his very next paper, "On the Mobility of Ions in Air at High Pressures," in the *Physical Review* for 1912, we find him embarking on the type of problem which broadened into his life work. Dempster used to tell an amusing story about his second paper. He had expressed the independent variable—pressure

*I have drawn very heavily for this biography on a Biographical Memoir prepared by Dr. Karl K. Darrow, sometimes quoting whole passages verbatim. . . SKA

of the air—in atmospheres to three or four significant figures, amply accurate for the purpose. His professor told him to convert these values into centimeters of mercury. Being about to leave for Europe, Dempster delegated this task to a fellow-student. The student converted the values and expressed them to *six* significant figures, and in this form they still stand in *Physical Review*.

In 1911 Dempster went to Germany for a sojourn of three years; he was one of the "1851 Exhibition Scholars," so many of whom have imprinted their names on the history of science. He went first to the Universities of Munich and Göttingen for a semester each, then to the University of Würzburg for two years. One would like to know why he chose Würzburg, for this was the decision which determined his whole career. The professor of physics at Würzburg, W. Wien, was engaged in researches which involved the deflection of beams of positive ions by electric and magnetic fields. This was precisely the technique suited to the separation of isotopes, though Wien was using it for the study of electrical discharges in gases, and so did Dempster in the work which was to have been his doctor's thesis. It was not to be his thesis, for Dempster's stay in Germany was suddenly terminated by the tragic first of August of 1914. A British subject, he was in imminent danger of arrest and internment; prudently he fled from Würzburg by the last train which carried civilians before the general mobilization. Another Canadian student made the opposite choice, and spent more than four years in the internment camp at Ruhleben.

Dempster thus found himself forced to recommence his progress toward the doctorate; and for this purpose he chose the University of Chicago. Here he arrived at the opening of the autumn quarter of 1914. It was immediately obvious that here was an outstanding man; and nobody was surprised when in 1916 the University conferred on him the doctorate with the grade *summa cum laude*, which was granted to only one other student of physics throughout the thirty years before the University abandoned the practice of giving gradations with the doctorate. After a period of service in the United

States Army during World War I, which involved his naturalization as a citizen of the United States, he returned to the University of Chicago in 1919 as Assistant Professor of Physics. He was happy at the University of Chicago, and the University was happy in having him; and from 1919 onward he never left it, except for vacations, one summer quarter in which he taught at Stanford University, and once in 1941, when he was briefly engaged in "classified" work at Washington.

In his new appointment at Chicago, Dempster immediately began a series of researches in physics which demonstrated his mastery of experimental technique and his acumen in selecting important problems for study. One may summarize his entire field by saying that practically every experiment performed by him involved a study of the properties of positive rays, or canal rays as they were known at the time he began his work. These are positively charged atoms or molecules set into motion at high speed through the application of electric fields, causing them to appear as a pencil or ray, often faintly luminous, moving through the residual gas in a vacuum. These beams, which normally move in straight lines, can be deviated or deflected by electrical or magnetic methods. The constituents of the beam having heavy mass are in general less easy to deflect than the lighter mass constituents, and thus the deviated beam will separate out into many smaller beams, each representing one of the masses in the original. The analysis of the constituents of a positive ray beam is known as mass spectroscopy, and this particular application of positive rays constituted Dr. Dempster's major effort.

The immediate precursor of the modern mass-spectrograph was the production of the so-called positive ray parabolas by J. J. Thomson in 1913. In this technique, positive rays from a partially evacuated vessel in which an electric glow discharge is taking place leave through a small cylindrical canal in the anode. After leaving the anode, they enter a second vessel at somewhat lower pressure, and pass through an electric field on which a magnetic field is superimposed, the lines of force of the two fields being parallel. The electric and magnetic deflections, thus produced, are at right angles, and inversely proportional

to, the energy and to the momentum of a single particle in the canal ray beam. On a photographic plate placed in a plane perpendicular to the axis of the canal, the ions of equal mass are spread into a parabola, and the Cartesian coordinates of any point in the curve give the energy and momentum of the particles which produced the developable image there. Thomson easily observed parabolas due to hydrogen, both atomic and molecular, to carbon monoxide and dioxide, and to mercury. In his classical experiment with natural neon, of atomic weight 20.183, he observed two closely spaced parabolas corresponding to masses 20 and 22, and drew the correct conclusion that natural neon is a mixture of isotopes each having very closely integral atomic weights.

This capital discovery must have stimulated Dempster's mind as it did all those who heard of it and who could appreciate its importance. It is small wonder that Dempster, beginning his career at this time, was attracted to this field. Dempster brought with him a formidable array of talents, some of which may be analyzed as follows:

- (1) He had a scholarly interest in physics which led him to read widely and become well oriented in the subject, thus being able to bring various fields of physics to bear upon any problem at hand.
- (2) He had an unusual technical ability in the laboratory. He delighted in devising mechanisms and instruments and showed considerable skill in what may be termed scientific invention. In too many cases, this type of ability is not associated with profound scholarship and it is in the combination of these that Dempster excelled.
- (3) His intelligence and foresight directed him toward the really important problems and thus he did not dissipate his time and energy on scientific trivia.

The impact of these qualities on the problems of mass spectroscopy was apparent as soon as he entered the field. He saw that J. J. Thomson's original device could be specialized into a true mass-spectrograph, having as its purpose the analysis of an ion beam into the various masses of which it was com-

posed. The parabolas gave more information than was called for; they gave both mass and energy distribution. Furthermore, they did this at the expense of intensity. If one wished merely to establish what masses were present, why distribute the particles of that mass out along a parabola; why not bring all particles of the same mass to a single position?

Actually, the improvement was carried out in two different ways, independently, by F. W. Aston, and by Dempster. Aston crossed the lines of force of the electric and magnetic fields at right angles, and applied them at successive positions in the path of the beam, rather than coincidentally. In this way he attained the result that all particles of the same mass and charge are brought to the same position on the photographic plate irrespective (within limits) of the velocity with which they emerge from the source. However, care must be taken that a beam of ions parallel in direction must be chosen for the analysis. The obvious description of this is velocity-focusing.

Dempster's scheme could properly be described as direction focusing. Here the long canal necessary to select for transmission and analysis only those positive rays traveling in an approximately parallel bundle can be dispensed with, and a divergent bundle of rays accepted, provided they are approximately mono-energetic. Dempster achieved the direction focusing by use of a homogeneous magnetic field which was constant over the region in which the ions were deflected, and by placing the defining entrance aperture and the detector 180° apart on the circular ion path. Instead of the conventional gas discharge as a source of ions, Dempster evaporated a solid deposit of the material to be analyzed from a platinum filament, producing ions by a concomitant electron bombardment. The production of the ions, and their acceleration, was carried out in relatively high vacuum, so that the desired homogeneity in energy was attained.

In this work, the innovation in the type of ion source was fully as important as the introduction of direction focusing to positive ray analysis. The gas discharge type of ion source was necessarily limited to those materials which could be prepared in gaseous form. Dempster was able to extend his re-

searches to solid sources, and it was due to his use of such solids that he was able, seventeen years later, to make his most important single isotopic discovery, that of U 235. He at once applied his new method of analysis to the study of lithium and magnesium, both difficult to prepare in gaseous compounds. He later made the first isotopic analyses of platinum, palladium, iridium and gold.

Many modern mass spectrographs contain both direction and velocity focusing, and Dempster was in the forefront of those constructing such instruments. Actually, in the period 1933-35 three double focusing spectrographs were in independent construction in three different laboratories. In 1935 Dempster was the first to publish the description of such a completed instrument. The double focusing was attained by the use of a cylindrical electrostatic field, for direction, and a subsequent magnetic field, which, if properly adjusted, gives velocity focusing. The mass lines were very sharp and developable images could be made with brief exposures. It was characteristic of Dempster that with this instrument, completed in 1935, he explored exhaustively the remainder of the periodic system, and was working with it at the time of his death. Others took up the new principles which he had helped to introduce, and constructed larger and more elaborate instruments, but he kept to his 1935 machine and extracted from it every bit of information which time allowed him.

Improvements in the source of the ions, however, were always underway. In the early 1920's, Professor R. A. Millikan, who was then on the faculty at Chicago, developed a "hot spark" light source for the enhancement of spectrum lines in the far ultraviolet. These lines arise from highly ionized, or "stripped" atoms, and Dempster was interested in the deflection of such multiply charged ions. His source development paralleled that of Millikan's, and the two stimulated each other. Dempster made exceptionally skillful use of the multiply charged ions of the heavier elements in his work on packing fractions from which the energy content of nuclei can be deduced.

In 1938 Dempster published probably his most important single paper, in which he summarized his results and those of

others into a discussion of "The Energy Content of the Heavy Nuclei." He showed clearly that the energy content per nucleon rose in the heavier elements, after passing through a minimum in the region near iron. This was made clear by the use of the multiply charged ions previously mentioned. For instance, if the masses of tin 119 and uranium 238 were exactly integral, the line for doubly charged U 238 should, in a mass spectrograph, coincide exactly with singly charged Sn 119. The fact that the lines do not, in fact, coincide enabled Dempster to supply the data which predicts the energy released when a heavy nucleus, like uranium, divides into two lighter nuclei of approximately half its weight. This is the process we call fission. Although he did not go so far as to predict the phenomenon of fission (and neither did any other physicist at that time), it was Dempster's result which showed that 196 million electron volts of energy per uranium nucleus would be given off in such a dissociation. This of course greatly enhanced the interest in the discovery.

When the so-called "Metallurgical Project" for the development of the military applications of atomic energy came to the University of Chicago, Professor Dempster's laboratory was immediately incorporated in the effort. The importance of the work was such that he was given financial assistance and experimental equipment such as had not been previously available. His joy at the new opportunities, which were continued in the postwar Argonne National Laboratory, was obvious. He remained on leave of absence from the department of physics until the time of his death, and worked every day in his laboratory. One of the many experiments he performed in later years was the detection of the isotope of cadmium which is responsible for the huge neutron absorbing power of the natural material. Cadmium which had been exposed to enormous neutron concentrations in the chain-reacting piles was furnished him, and his mass spectrograph showed at once which of the many isotopes had been transformed by the absorption of neutrons. Similar experiments were performed to detect the neutron-absorbing isotopes of samarium and gadolinium. These beautiful results have already appeared as

illustrative material in textbooks on nuclear physics and will be so used for many years.

Although Dempster will be remembered chiefly as an expert in the field of mass-spectroscopy, his researches and his scholarly interests in physics had broader horizons. In an estimate of his own work, which he prepared in 1945 in connection with the honorary degree of D.Sc. which the University of Toronto awarded him shortly thereafter, he lists six other fields of investigation to which he made contributions. These were:

- (1) The mobility of ions in air at high pressure
- (2) The broadening of spectrum lines by pressure of helium gas
- (3) The ionization products formed in gases by pure electron impact
- (4) The light excited and emitted by positive ions of various energies
- (5) Radiation processes and interference of single light quanta
- (6) The passage of protons and other ions through helium gas.

Looked at in retrospect, however, we see that these researches were either digressions from the main path, or preliminary researches before he became engrossed in mass-spectroscopy. None of them was ever pushed far enough to result in a major contribution. In 1929 he spent considerable time investigating the reflection of a proton beam by calcite and other crystals when the beam fell on a cleaved face at a very small glancing angle. In some of his early experiments, a pattern appeared in the beam leaving the crystal face, and he cautiously advanced the suggestion that it might be a diffraction pattern, and might be direct evidence that the motion of protons is governed by quantum, rather than classical, mechanics. The experiments of Davisson and Germer, showing a similar effect for electrons, had been published two years previously. Further experiments, however, showed that the patterns were complex and varied from crystal to crystal. He finally abandoned this work, after

convincing himself that no simple explanation, based on diffraction, was possible.

Professor Dempster's contributions to physics were recognized by many honorary awards. He was elected to the National Academy of Sciences in 1937. In 1932 he was made a member of the American Philosophical Society, which conferred on him its Lewis Award and its Glasham Gold Medal, to his great pleasure. In 1944, he became President of the American Physical Society. His own university, Toronto, conferred on him the honorary degree of D.Sc. in 1947, an occasion which was attended and enjoyed by his many relatives in that city. At the time of his death he was Professor of Physics in the Division of the Physical Sciences of the University of Chicago, and Director of the Argonne National Laboratory's Division of Mass Spectroscopy and Crystallography.

Although until his participation in the national effort on atomic energy he regularly taught classes in the standard subjects of physics, he never became at ease in the classroom or on the lecture platform. He did not have the gift of smoothness and lucidity in verbal presentation, and he was himself well aware of this lack. He set very high standards of scholarship for himself, and respected and admired high scholarship and intellectual activity in others. He had a keen interest in University affairs and strong opinions concerning them, especially the role of the faculty in University administration. He was sure that the faculty, and only the faculty, was the competent judge of fitness for University appointments. These opinions were fearlessly maintained and vigorously advocated. Experience at Chicago and in other universities has shown that continuous presentation of this point of view is beneficial in maintaining the principle involved.

In his private life, outside his laboratory and his classroom, he was a man with a very small circle of intimate friends. During his visit to Stanford University, in 1923, he met Germaine Collette, a Belgian scholar from the University of Liege, who was studying medieval literature at Stanford. Their friendship resulted in marriage in 1926. His wife, the holder of Ph.D. degrees from Liege and Stanford, continued her career beside

him and has become an outstanding contributor to Chaucerian criticism. Her researches never failed to arouse the sympathetic interest of her husband, who greatly admired and appreciated her work.

Dempster was widely read in the fields of economics and philosophy and had a lively interest in the history of science. He and his wife enjoyed travelling and spent many of his free periods on trips which most often included the countries of continental Europe, but were also extended to Mexico, Egypt, and Guatemala. His comments on the political economy and customs of the countries visited in his travels were eagerly awaited by his friends. In between his longer trips he was fond of shorter excursions from Chicago, always into places where he could walk in the open country. Some of his haunts were Saugatuck, Michigan and the Great Smoky Mountains National Park.

In politics he was staunchly conservative. He was never dismayed to find himself holding opinions widely at variance with those of most of his professional colleagues. Surprisingly enough, he took no objection to, and even approved of, the rigid secrecy imposed by the Government on certain wartime and postwar researches, at a time when many scientists were outraged at what seemed to them the excesses to which the security program was being pushed. When on leave from the University and conducting his researches under the auspices of the Argonne National Laboratory of the Atomic Energy Commission, he was asked whether he would be willing to continue research, granted the very favorable conditions which the Argonne Laboratory was able to offer, but contingent on the stipulation that none of his work would ever be published to the world at large, he answered vigorously in the affirmative.

Physically, Dempster was of medium stature, slender, black-haired, and of finely chiseled features which an artist would have been glad to adopt as the embodiment of the intellectual man. His hands were large, and well and sensitively formed. His appearance and alert demeanor changed little with the years, and he died essentially a young man. His first attack of coronary thrombosis, occurring in June 1948, compelled him

to a careful and restricted mode of life; and surely there was never a patient who so meticulously followed the advice of his physicians. The second and final attack was fortunately brief in duration. It occurred on a vacation in Florida and his friends have all seen photographs taken the day before the end, showing him at ease in his informal clothes, enjoying the warm sun and balmy air at a modest outing-place there.

His marriage did not result in children. His wife, surviving him, continues her scholarly career. The National Academy lost in him one of its ablest and most respected members.

KEY TO ABBREVIATIONS

- Amer. Jour. Phys. = American Journal of Physics.
 Astrophys. Jour. = Astrophysical Journal.
 Bull. Amer. Phys. Soc. = Bulletin, American Physical Society.
 Jour. Roy. Astron. Soc. Can. = Journal, Royal Astronomical Society of Canada.
 Philos. Mag. = Philosophical Magazine.
 Phys. Rev. = Physical Review.
 Phys. Zeit. = Physikalische Zeitschrift.
 Proc. Amer. Philos. Soc. = Proceedings, American Philosophical Society
 Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences.
 Rev. Mod. Phys. = Review of Modern Physics.
 R. S. I. = Review of Scientific Instruments.
 Sci. Mo. = Scientific Monthly.

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Chesley Stark

CHESTER STOCK

1892-1950

BY GEORGE GAYLORD SIMPSON

The flavor of Chester Stock's life and the eminence of his career derive from his personal charm and from an unusually even balance of teaching, administration, and research in his profession. It is impossible to point to a unique trait and say, "This is what made him so unforgettable," or to name one phase of his work and say, "This is what made him a great paleontologist." The meaning of so broadly integrated a life is hard, perhaps impossible, to convey fully because no single aspect dominates. The problem is to portray the whole man, and even with help from many sides this is a difficult, complex task.

Ancestry and Boyhood

Although Chester's parents met and married in San Francisco, both came from Germany. John Englebert Stock was born on November 5, 1857 in Stadt-Orb, Hessen-Nassau, where his father was a well-to-do merchant. His mother, whose maiden name was Lour, was the daughter of a Bavarian forester. John Englebert emigrated to America in 1879, going first to Chicago and then in 1882 to San Francisco, where he engaged in the wholesale liquor business and died in 1914.

Maria (Johanna) Henriette Meyer, Chester's mother, was born May 6, 1860 in Geesendorf, Bremerhafen. Her grandparents on both sides were landed proprietors, in Oldenburg and in Hanover. With her family, she emigrated to New York City in 1875 and moved to San Francisco in 1884. There she married John Englebert Stock on November 9, 1885. She died in 1921.

To this couple Chester Stock was born in San Francisco on January 28, 1892. The family's circumstances were humble and they lived in the poorer section of the city. The Barbary Coast was more than a legend then and young Chester early became acquainted with the seamy side of life while roaming the streets with the other boys. Some of these companions were

rough characters and "Red" Stock was a frequent scapegoat for their deviltry. He laid the foundation for a true Horatio Alger story by selling newspapers. His usual stand was at the corner of California and Battery streets and one of the memories of a hard boyhood was of being roundly beaten up in the defense of this corner against a larger newsboy.

In spite of these tough surroundings, Chester early showed the makings of a scholar. He attended kindergarten, Starr King Primary School, Franklin Grammar School, and Polytechnic High School. Even the week-ends were busy, for on Saturday he went to gymnasium and was tutored in German and he was a regular attendant at the Lutheran Sunday school and church. On top of all this, time was found for music lessons on the tuba. The lessons were accompanied by encouraging sips of wine provided by the "professor," and the small boy used to wonder why tuba lessons made him feel a bit queer and dizzy. He played in the band of the Columbia Park Boys Club and must have acquired considerable proficiency for he later also filled professional engagements. Most remunerative of these were in the funeral processions of local bigwigs, for which the tuba player received the munificent sum (as it really was then) of \$5.00.

Stock continued playing in bands at dances, excursions, and games into his sophomore year in college but then dropped music because study took all his time. In later life he sometimes expressed regrets that he had not carried his musical interests further and at one time he planned to learn to play the piano, but this, too, was dropped. J. R. Schultz recalls a session while he was a student of Stock's at which he played some Schoenberg records for Stock, who was puzzled by them. The evolution of this music from that of Beethoven and other classical composers was explained. Stock became greatly interested and drew parallels between the development of music and of vertebrate paleontology. On the whole, however, music had no abiding significance for Stock.

This was not true of some other boyhood interests, which did foreshadow his lifelong preoccupation. His father took him to the annual Mechanics Fair where he was spellbound by the

displays, especially those of mining activities. He also frequented the California Academy of Sciences, then on Market Street between Fourth and Fifth. The black-painted models of an ichthyosaur and plesiosaur struck him with never-forgotten awe and he especially mourned the loss of the mammoth when it was destroyed by fire in 1906. Great was his relief when his teacher explained that science had not suffered irrevocable damage in the loss of a creature constructed largely of two-by-fours generously padded out with hemp.

The great earthquake and fire of 1906 held more traumatic memories for the young future paleontologist. The Stock home burned and the family fled to a park high up in the city, where they camped for three weeks. Although only 14, Chester was mustered into the California National Guard during the emergency.

The elder Stocks were crushed by this experience and Chester's father apparently never fully recovered from it. Chester, himself, was left with a deep-seated fear of fire revealed in later years by his forbidding smoking above the first floor of his home and by little habits such as frequently emptying ash trays into fire-proof containers.

The effect on already shaky family finances was equally disastrous. The Stock house had been insured in a German firm which failed to pay its losses, and the elder Stock's business also received a fatal blow. Chester left high school and went to work in the Old Union Iron Works. Here his health soon broke down under the extremely hard labor and an attack of malaria. In this crisis, his mother, always a tower of strength, and his brother determined that the best hope for the studious boy was to go back to school with their aid and eventually to enter a profession. He entered Mission High School and was graduated with honors in 1910. The increasingly scientific bent of his mind is revealed by the title of his essay in the school yearbook: "Sleeping Sickness—The Tsetse Fly."

Student Days and Early Career

In the fall of 1910 Chester entered the University of California (Berkeley) with the intention of studying medicine. It is

not clear precisely how he came to transfer his ambitions from medicine to vertebrate paleontology. His brother remembers the decision as having been made reluctantly and as due to the financial burden on his family. On the other hand, in later years Chester, himself, mentioned to several colleagues that Professor John C. Merriam induced and enabled him to become a paleontologist, and he said at least once that the turning point came when Merriam sent him to Hawver Cave to collect Pleistocene mammals, an occupation with which he became fascinated. The most probable interpretation is that opportunity for earlier self-support was welcome and that Merriam provided such an opportunity and aroused interest, but that the really decisive factor for Chester, as for many other research scientists, was the discovery that he could advance scientific knowledge with his own hands and brains.

Merriam's influence on Stock was profound and it went far beyond the fact that Merriam was the means of recruiting Stock into the ranks of vertebrate paleontologists. Stock's life and work cannot be fully understood without recognition of an odd sort of intellectual symbiosis with his teacher, against which he sometimes fretted but which he recognized as important and helpful. Merriam was not always intellectually generous or fully appreciative toward Stock and it is clear that he sometimes hurt Stock deeply, yet each of them felt some measure of dependence on the other. These undercurrents did not spoil the relationship of devoted student to respected teacher.

Merriam's interests in geology and paleontology were remarkably broad, but when Stock became his student he was particularly concerned with two main subjects: the extraordinary Pleistocene mammalian fauna of Rancho La Brea (in Los Angeles, California) and the paleontology and correlation of numerous, scattered, mammal-bearing Cenozoic formations in the Great Basin and Pacific Coast regions. Stock began work on both subjects under Merriam and practically the whole of his research career was confined to these or closely related topics. His approach to them was also, and remained, like Merriam's, an excellent model.

The Hawver Cave collection, previously mentioned, was

assigned to Chester for his Ph.D. dissertation, duly published in 1918. In the meantime, however, and while Chester was still an undergraduate Merriam also turned over to him the study of one of the most striking and abundant groups in the Rancho La Brea fauna, that of the ground sloths. Stock's first published papers, three of them written before he was graduated, were on this subject. These studies eventuated in his major publication, the large memoir of 1925. This was published by the Carnegie Institution of Washington, of which Merriam was then president. The Institution continued to support and to publish much of Stock's work and that of his students through most of his career. Stock was a research associate of the Institution from 1926 to 1944.

Chester's first experience with field work gave him a lifelong enthusiasm for it. Early work in Nevada (1919) and Oregon (1920) was rugged and left many memories in the minds of Chester's companions. Something of the flavor of those expeditions may be suggested by quotation of two (among many) of R. J. Russell's anecdotes of the 1919 field season.

"The Mormons of southeastern Nevada are the most hospitable of all people. Instead of being outcasts, bums, suspicious characters, or whatever our dress and habits might indicate, we were accepted. A child would appear with a chocolate cake, jar of honey, jar of cream, or other gift, sent over by her mother. The Mormons are mild mannered, too. We established camp at the side of a ditch after several weeks without any surplus water. Here was a chance not only to get ourselves clean but also to do the accumulated laundry. Next morning a hay wagon stopped at camp. 'Boys, we would rather that you wouldn't wash your clothes in the ditch. We drink that water, and last night it was pretty soapy.' . . .

"Fossil hunting was terrible along Muddy Valley. Day after day, and in terrific heat, we would walk along the topographic complexities of the badlands, Chet on one ledge and I on another nearby. At length a prayer would be heard from Chet, a prayer of request starting with a petition for the complete skeleton of some extremely rare Tertiary mammal. In continuation it would ask for a jaw, for a tooth, for a tooth fragment, for a leg

bone, a vertebra, and eventually terminate with some such statement as, 'If Thou seest not fit to reward Thy sweating but humble servants, the lowly paleontologists who scan with care each mineral grain of Thy creation under the rays of Thy ever-shining sun, with even the astragalus of a camel, wilt Thou grant unto Thy humble supplicants the pleasure of finding at least one bone fragment, one splinter, or the tiniest chip of some animal creation which once lived, loved, and roamed in these Thy broad dominions.' "

All bone-diggers know that mood, and all know, too, that any discomfort is forgotten when a find is made and that the call of the badlands is eternal and irresistible. Chester continued throughout his life to direct an energetic collecting campaign in which he took as frequent a personal part as circumstances and other duties would permit. In common with all really successful collectors he had what can only be called a lust for fossil bones. Through personal contacts, popular writing, and lecturing he turned most of the oil geologists and many of the ranchers of the Southwest into his scouts in the search for leads to new fossil localities, leads always vigorously followed up by him or his students.

Chester was graduated with a B.S. in 1914 and received his Ph.D. in 1917. Merriam kept him on as an assistant (1917-1919) and instructor (1919-1921). In 1921 Merriam left the university and went to Washington as president of the Carnegie Institution. Stock, as assistant professor, became his successor in the teaching of vertebrate paleontology at Berkeley, a post in which he continued for five years. During this time he completed the memoir on ground sloths from Rancho La Brea, took over a monograph planned by Merriam on the saber-tooths and cats of the same fossil deposit, and made numerous shorter studies of western Cenozoic mammals and their occurrences.

Later Career

While Chester was teaching at Berkeley, the California Institute of Technology, in Pasadena, was expanding its program under R. A. Millikan, and the formation of a Division of

Geological Sciences was planned. Merriam recommended J. P. Buwalda to head the proposed division and when Millikan went to Berkeley to interview Buwalda he also became deeply impressed by Stock, whom he had met casually at the Los Angeles County Museum some time before. The upshot was that both Buwalda and Stock moved to Caltech and that around them was developed one of the great geological departments of the country: Chester, then 34, was given full professional rank and continued in that capacity until his death. In 1947 he succeeded Buwalda as Chairman of the Division of Geological Sciences. The division flourished under his brief chairmanship and it is significant that the physical aspects of geology, in which he had least purely personal interest, were especially advanced.

Chester had married Clara Margaret Doud on June 2, 1921 and he took to Pasadena with him his wife and their three-year-old daughter, Jane Henriette (now Mrs. J. R. Sullivan). A son, John Chester, was born soon after the move. Mrs. Stock's death on March 20, 1934 ended a deeply-felt relationship and was one of the tragedies of Chester's life. On July 3, 1935 he re-established a happy home-life by marrying Margaret Gardner Wood, who had been associated with the administrative offices of the Institute. Their son, James Ellery, was born in 1942.

Although most of his time was devoted with great success to teaching, field work, and research at Caltech, Chester really had several related careers after moving to Pasadena. Among these, his connection with the Los Angeles Museum was second in importance only to that with Caltech. A large part of the great Rancho La Brea collections is in the Los Angeles Museum and this had brought Chester there to work during university vacations as early as 1918. After 1926 his home base was within easy reach of the Museum and he thereafter spent much time there in research and in supervision and development of paleontological and other activities. He was successively Curatorial Consultant (1931-1939), Senior Curator of Earth Sciences (1939-1948), and Chief Curator of Science (1948-1950) in the Museum.

One of Chester's consuming interests was the development

by the Museum and the county of Rancho La Brea, Hancock Park, as an attractive and instructive scientific center where prehistoric animals could be seen at one place in their original burial ground, in reconstructed skeletons, and in life-like restorations. Years were devoted to planning and preparation. Some of the landscaping was completed, a pit was dug to show bones in place, and an observation station was erected around the pit. Chester spent the afternoon before his death conferring with the director of the Museum and the architects on the next steps: completion of the landscaping and building of a museum in the park to house the Los Angeles Museum's collection of Rancho La Brea fossils. This plan is still expected to be carried out.

In 1935 Chester became particularly interested in paleontological work in Mexico, where parties under his direction worked almost continuously until the time of his death, except for a short interruption during the war. Great as was his interest in the large, important fossil collections obtained, Chester was at least equally interested in the development of friendly relationships and scientific cooperation between Mexico and the United States. This cause became a fervent crusade for him and increasingly occupied his time and attention. (His presidential address to the Geological Society of America, which would have been his most important public appearance but which he did not live to make, was to have been on this subject.) The work was done in cooperation with the Geological Institute of the National University of Mexico and the collections are being divided between that organization and the California Institute. It was planned that research and publication should eventually also be shared by the two institutes and an important paper on fossil horses by Stock's student J. F. Lance has been published in Spanish in Mexico.

Besides the positions already mentioned, Stock was visiting professor at the University of California at Los Angeles in 1939, and in 1943-1945 he held a war service appointment as senior geologist with the U. S. Geological Survey, working in the Los Angeles Basin. From 1948 until his death he also had a U.S.G.S. appointment (on a "w.a.e.," "when actually

employed," basis) and worked with D. F. Hewett on Mohave Desert Tertiary faunas.

Chester was active in the work of various scientific organizations and especially in the Geological Society of America, which he served faithfully in various capacities for years and by which he was elected president in 1950, shortly before his death. He had previously been president of both of the other two national societies including his field, the Paleontological Society in 1945 and the Society of Vertebrate Paleontology in 1947. He was also a member of the National Academy of Sciences, the American Philosophical Society, the American Academy of Arts and Sciences, the American Association of Petroleum Geologists, The American Society of Naturalists, and several other scientific and professional organizations.

Such are some of the facts of the distinguished and useful life that ended suddenly on December 7, 1950. Fuller comprehension requires some appraisal of Chester Stock as a teacher, as a research scientist, and as a person.

Teaching

"Chet's first experience teaching a class [in 1921] was painful, both to him and to the students. . . . [He] had done an immense amount of preparation but was visibly frightened and delivered his talks in a monotone that soon put each class to sleep. As he told the experience at a later date, 'It was rotten. . . . Five students asleep, completely indifferent to the complexities of the development of the mammalian dentition and but one awake, taking notes studiously. . . . Well, it may be worth while; one student is getting it. . . . Yes, one bit of fertile soil justifies planting the seeds. But on the last day I managed to peek at the one student's notebook and what did I see? He was drawing hundreds of tiny men, rows and rows of tiny men. . . .'"—So remembers R. J. Russell.

From this inauspicious beginning developed one of the most successful and best-loved teachers of paleontology. Let some of his colleagues and students bear witness:

"As a teacher, Dr. Stock possessed the enviable ability to transmit his enthusiasm for the subject to the student. By some

strange magic of voice and expression, he made it seem to us that the presence or absence of some slight structure on a fossil jaw was of vital importance even if we were not all prospective paleontologists. . . . His imparting of solid fact would be interrupted frequently by departures into a tale of some adventure associated with the finding of a particular specimen. Delightful as all this was to the student, it sometimes led to academic disaster for the story would remind Stock of something else, and that of something else. On at least one occasion, there was a long pause after the conclusion of the last story. Finally Stock muttered, 'Just where the hell was I?' . . . He believed that education was an internal process. . . . He made certain that we were grounded thoroughly in morphology and were fired by his own enthusiasm. The rest was up to us."—R. W. Wilson.

In Stock's annual lecture to a large elementary class, "the first ten or fifteen minutes were always uproarious as Chester rolled them in the aisles. . . . He told of barefooted sailors in his classes, of eyeless moles, and of not so eyeless co-eds, and always ended with an entreaty for 'you bozos' not to forget Chester Stock and Caltech after making 'your first million bucks.' From this beginning . . . Chester passed gradually into a beautifully philosophical lecture on the principles of geological history."—R. P. Sharp.

"His lecture methods were informal and effective . . . [but] he was at his best in discussion and conversation. His lectures were interspersed with amusing anecdotes of the great pioneers in paleontology. . . . He kept an extensive file of what he called 'Americana' and it was worth something to hear him read Mark Twain's theory of the origin of the fossil footprints in the Carson City prison yard."—J. R. Schultz.

"Chester's impact on the student was terrific. . . . He could spellbind a class of three or four students or an audience of hundreds with equal ease."—P. C. Henshaw.

"As a teacher, Chester was one of the most stimulating individuals on the campus. . . . His audience never knew what to expect from one moment to the next, and no one ever slept through one of his lectures. . . . By far his greatest contribu-

tions in teaching, however, were at the advanced levels. . . . He left his stamp on literally dozens of men, and in a way much more fundamental than might be expected from a man working in such a specialized field. . . . Many . . . were introduced to the techniques of research. . . . These men were taught the basic discipline of good descriptive interpretive writing. . . .” —R. H. Jahns.

It is estimated that about 1,525 students passed through Chester’s classes at Caltech. Few of these majored in vertebrate paleontology and fewer still continued in that science after leaving the Institute, but all were left with a feeling, which even some hardrock geologists call “love,” for the subject. J. R. Schultz adds that “in such diverse places as India, Europe, and Central America when it was found that I had studied at Caltech the question was, ‘Then you must know something about vertebrate paleontology, for everyone who comes from there seems to.’”

Vertebrate paleontology is a small and not strikingly remunerative profession, so even among Chester’s major students, those who wrote dissertations under him, entered some other field. Most of them are now successful geologists in industry and government service, although a few are outstanding vertebrate paleontologists, such as C. L. Gazin of the U. S. National Museum and R. W. Wilson of the University of Kansas. It cannot be said that Stock’s teaching established a trend or developed a school. As a teacher he was less a trainer of specialists than an ambassador for his science, for science in general, and for intellectual curiosity and integrity.

Research

With rare exceptions, Stock restricted his own research to a narrow and unified field of specialization: the fossil mammals of western North America and the deposits in which they occur. His research approach was quite rigidly factual. He described what he saw, with as little interpretation as is consistent with the marshaling and use of facts, with no speculation, no theorizing, and no philosophizing. He devised no methods, advanced no theories, made no broad syntheses, did nothing to

change the procedures or attitudes of his science. As he, himself, once put it, "I'm after the facts and I'll let the other fellows go wrong trying to guess what they mean." Thus he was in his research, although his unpublished musings might be deeply philosophical. He had, for instance, a lifelong preoccupation, almost an obsession, for the problem of the origin of life.

At the level where he chose to work, he worked extremely well. His research papers are often masterpieces of succinct, clear, accurate, objective exposition. He added greatly to the essential basic data of paleontology and stratigraphy. His published productions include 2 large monographs and 169 shorter works, mostly technical research but including a few popular articles and summaries.

"Cenozoic Gravigrade Edentates of Western North America, with Special Reference to the Pleistocene Megalonychinae and Mylodontidae of Rancho La Brea," published in 1925, was Stock's largest independent publication. It made him the recognized authority on North American ground sloths and was so thorough that no really substantial further contribution to this subject has yet been made. (At the time of his death Stock had in hand but had not yet completed an important supplementary study on some earlier North American ground sloths.) The bulk of the monograph (154 of its 201 text pages) is devoted to painstaking anatomical description of the Rancho La Brea specimens. The scanty ground sloth specimens from elsewhere in western United States were also described, and briefer notice was given to distribution of the group, probable habits and habitats of these animals, and their classification. The whole is a model of the sort of solid study that forms the factual basis of the science of vertebrate paleontology.

Before leaving the University of California Merriam had completed several studies on parts of the Rancho La Brea fauna and had planned a large monograph on the Felidae. The felid material, even richer and more spectacular than that of the ground sloths, is especially noteworthy for the sabertooth, *Smilodon californicus*, and the giant jaguar, *Panthera atrox*. It is a result of the work of Merriam and Stock that these are among the most widely known of prehistoric animals. When Merriam

became president of the Carnegie Institution of Washington, administrative duties greatly curtailed his original research and the writing of the monograph of the Felidae was turned over to Stock. It was issued by the Carnegie Institution in 1932 as a joint publication by Merriam and Stock. In approach and style this large volume, "The Felidae of Rancho La Brea," is closely similar to Stock's ground sloth monograph: a painstaking description of the La Brea specimens, supplemented by descriptions of some similar materials from elsewhere in western United States and by remarks on distribution, habits and habitat, and classification. This publication, too, ranks as a solid, factual contribution and is the definitive work in its field.

Stock's other publications were all much briefer than his two monographs, but conjointly they bulk large and include much that was and is of basic importance. While still working on the Rancho La Brea collections, Stock (1918) described a Pleistocene mammalian fauna from Hawver Cave, in California. There was some but, as it turned out, insufficient evidence of the association of man with this fauna, and its study initiated Stock's continuing interest both in cave faunas and in early man in America. Among other studies in these fields, he described fossils from Shelter Cave, New Mexico (1930, 1932) and discussed problems of antiquity of human remains or artifacts from Los Angeles (1924), Gypsum Cave, Nevada (1931), and Clovis, New Mexico (1936). Among his few papers that review a general topic rather than describe a particular occurrence are one on Pleistocene faunal sequence as related to early man (1936) and one summarizing literature on prehistoric archaeology (1941).

Stock imbibed from Merriam and passed on to his own students an orientation more geological than prevailed among most eastern American vertebrate paleontologists, where H. F. Osborn's biological approach had stronger influence. A considerable number of Stock's papers are devoted to fragmentary fossil mammalian discoveries the interest of which is less in the fossils, themselves, than in their bearing on distributional and stratigraphic problems. Work on such materials is peculiarly demanding and their use involves the highest degree of skill in

comparative morphology. Each of these numerous studies added a specific item to the complex sequence of Cenozoic strata in the western part of the Great Basin or along the Pacific Coast. The list of formations and local faunas fitted into that sequence by Stock and his students, whom he also supervised in much work of this sort, is long: Tecuya, Siesta, Panaca, Muddy Valley, Ricardo, Puente, Titus Canyon, and others. This sort of study was begun while Stock was an instructor at Berkeley and continued throughout his life. Aside from published results, he became an authority on western Cenozoic stratigraphy and was constantly consulted on this subject by other students and by commercial geologists.

Both from a paleontological and from a geological point of view, Stock's most remarkable work after he had gone to Pasadena and had completed the Rancho La Brea monographs related to the Sespe beds, a series of varicolored continental strata some 6000 to 7000 feet in thickness in the Coast Range and the Santa Monica and Santa Ana Mountains of southern California. In twenty-five papers, from 1930 to 1938 (most of them in the Proceedings of the National Academy of Sciences), Stock described fossil mammals from the Sespe. Four distinct ages were finally represented by these finds within the Sespe, ranging from late Eocene to early Miocene. This work was of outstanding importance in several different ways.

(1) The hitherto unknown ages of the various subdivisions of this imposing pile of sediments were determined.

(2) Interfingering of the Sespe with marine sediments (Tejon to Vaqueros) permits direct correlation of the marine invertebrate and continental mammalian faunal sequences. Such correlation had previously been impossible for this part of the geologic column. Two distinct time systems are in use, one for marine and one for continental sediments, and the relationships between the two could not be directly determined before Stock's work on the Sespe, which thus marked a great advance toward solution of this crucial stratigraphic problem.

(3) Eocene mammals had previously been unknown west of the Rocky Mountain states and early to middle Oligocene mammals poorly and scantily known west of the High Plains.

The Sespe discoveries extended such knowledge hundreds of miles westward to the coast and made a major contribution to the geographic distribution of early mammals.

(4) Most of the Sespe mammals belonged to new genera or species, so that these discoveries added materially to knowledge of faunal variety and differentiation. In all, Stock described and named the following numbers of new groups from the Sespe:

Order	New genera	New subgenera	New species
Marsupialia	..	..	I
Insectivora.....	3	..	4
Primates.....	3	..	3
Rodentia.....	I	..	3
Carnivora.....	I	3	II
Condylarthra	..	..	I
Perissodactyla	I	..	4
Artiodactyla	2	2	7
	—	—	—
Totals.....	II	5	34

The mammals were diagnosed in short papers each devoted to a single fossil or to a small number of related fossils, and no general listing or review of the faunas was made. Their stratigraphic significance was, however, briefly outlined in Stock's presidential address to the Society of Vertebrate Paleontology.

After the Sespe discoveries and especially during his last decade, Stock's main research interest was in the late Tertiary and Pleistocene mammals of northern Mexico. Conditions in the field did not permit his spending much time there and he was unable to complete much of the study of the extensive resulting collections, but he worked extensively on these and directed the whole program. Major excavations in high, remote San Josecito Cave, in Nuevo Leon, extended from 1935 to 1942 and produced thousands of bones of birds and mammals. Most of these were assigned to Stock's associates for study and several papers have been published on them although full lists and descriptions have not yet appeared.

At intervals from 1936 to 1950 much collecting was also done in the vicinity of Rincón and Yepomera in western Chihuahua. Here a rich mid-Pliocene fauna occurs, and here, also, thousands of bones were collected, including such novel forms as a six-horned antelope and a number of different early horses of unusual interest. Much of the preliminary preparation had been completed and a few groups had been studied by his colleagues and associates at the time of Stock's death but, again, most of the collection is a legacy for future research.

Besides the results of his technical research, Stock wrote an excellent, widely-read popular account of Rancho La Brea (1930, revised editions 1942, 1946, 1949) and, with Hildegarde Howard, a pamphlet on the evolution of the horse family (1944), both for the Los Angeles County Museum. He was also for a time a frequent contributor of short popular notes on geology and extinct animals in *Westways* magazine.

Stock had done much work on a textbook of paleontology which he left sufficiently advanced that D. P. Willoughby and C. W. Merriam plan to complete and publish it. All the illustrations and about two-thirds of the text of a monograph on the ground sloths of Florida were complete. Books had been planned and some text written on the geology of California and on the "Dark Mirrors," the tar pits of Rancho La Brea.

Personality

Stock was a robust, well-built man. He had sandy-red hair, which inevitably gave him a childhood nickname, and which retreated but little in middle age. His complexion was fair and became florid. He had a small mustache and wore rimless glasses over eyes usually bright with interest and humor. His hearing slowly failed over a period of years, and in the last six or seven years he wore a hearing aid, without which he was completely deaf. As a young man, in spite of one or two severe illnesses, he was highly active and vigorous. Companions of early field trips speak of his tirelessness. In late years high blood pressure forced some restraint, and he began to tire more easily; but he still gave an impression of ill-suppressed energy. In other respects his health was generally good, al-

though paroxysms of sneezing from hay fever occasionally evoked good-natured oaths.

The personal trait always mentioned by anyone who speaks of Chester Stock is good humor or cheerfulness. He remembered his parents as gay and fun-loving and remarked that this must have been because they were Bavarians and not stolid, serious Germans. In spite of his difficult childhood, they seem to have given this characteristic to him. His life was touched by deep sorrow more than once, but he passed on only happiness to others and had strong capacity for recovery and for enjoyment of life, profession, family, and friends. He was a sworn enemy of pomposity (as is hinted by some of the preceding comments on his teaching), and when forced into some semblance of official dignity would complain that "the bozos are trying to make a damned stuffed shirt out of me." His lack of pedantry was accompanied by an equal lack of egotism. He had no evident inferiority feelings, but he had genuine humility.

Chester worked well in organizations and smoothed administrative problems because he was ready to see other points of view and unwilling to indulge in controversy, either scientific or personal. He had a way of removing tension between others by jocular remarks inoffensive to all sides. He could be critical and had a few pet peeves against colleagues, past and current, but he never expressed criticism when or in such a way that it would damage or rankle. One of his ways to stop a controversy was to say, "I'm just a poor dumb paleontologist who doesn't know what you're talking about, but couldn't we get together and do this . . ."—and often everyone found that they could.

A faculty associate much younger than Chester (A. E. J. Engel) remarks that with him "Chester could scrutinize university policy, a research program, or a pretty girl in much the same close, happy way an old schoolmate and boyhood chum might. . . . His remarks were often punctuated with humorous, sometimes earthy analogies, or with delightfully and innocuously profane references."

In company Chester could be quiet, a courteous good listener, or could quite suddenly become the life of the party, a transi-

tion bewildering to those who did not know him well and, indeed, occasionally to those who did. He was sensitive to the mood of others, a truly social being in the best sense of the words.

Chester had few interests outside of fossils and people, the two subjects that absorbed virtually his whole attention. He avidly collected books connected with his profession. He enjoyed most of the innocent earthly pleasures, drinking with moderation and appreciating good food, especially the Spanish cuisine. He claimed no particular taste for the more abstruse music or literature, but greatly enjoyed dancing to Latin American music or reciting the poems of Bret Harte.

A candid appraisal must conclude that Chester was a good, sound, and useful scientist but not, in the usual sense, a truly great one. Yet everyone who knew him will agree that he was a great man. The paradox, if there is one, is resolved by the fact that so many whom he has left say, "I loved him."

Sources and Acknowledgments

The outpouring of materials for this account is, in itself, testimony to the affection and esteem in which Chester Stock was held. Mrs. Chester Stock has been cordially helpful. Mrs. Madeline F. Harding, Stock's secretary, provided the bibliography and obtained and compiled much other information, including most of the statements received. Information on Stock's career and personal statements of incidents and impressions were provided by A. C. Stock, Chester's brother, and by the following associates and former students: R. W. Chaney, A. E. J. Engel, P. C. Henshaw, H. Howard, R. H. Jahns, R. A. Millikan, H. H. G. Nash, R. J. Russell, J. R. Schultz, R. P. Sharp, and R. W. Wilson. I have also read all of Stock's publications and have drawn on my own prized memories of him.

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KEY TO ABBREVIATIONS

- Amer. Jour. Sci. = American Journal of Science
 Amer. Nat. = American Naturalist
 Bull. Amer. Assoc. Petrol. Geol. = Bulletin, American Association of Petroleum Geologists
 Bull. Calif. Inst. Tech. = Bulletin, California Institute of Technology
 Bull. Geol. Soc. Amer. = Bulletin, Geological Society of America
 Bull. S. Calif. Acad. Sci. = Bulletin, Southern California Academy of Sciences
 Eng. Sci. Mo. = Engineering and Science Monthly
 Jour. Geol. = Journal of Geology
 Jour. Mam. = Journal of Mammalogy
 Jour. Wash. Acad. Sci. = Journal, Washington Academy of Sciences
 Nat. Acad. Sci. Biogr. Mem. = National Academy of Sciences Biographical Memoirs
 Proc. Acad. Nat. Sci. Phila. = Proceedings, Academy of Natural Sciences of Philadelphia
 Proc. Nat. Acad. Sci. = Proceedings, National Academy of Sciences
 Sci. Mo. = Scientific Monthly
 Soc. Vert. Paleont. = Society of Vertebrate Paleontology
 Trans. San Diego Soc. Nat. Hist. = Transactions, San Diego Society of Natural History
 Univ. Calif. Publ., Bull. Dept. Geol. = University of California Publications, Bulletin, Department of Geology

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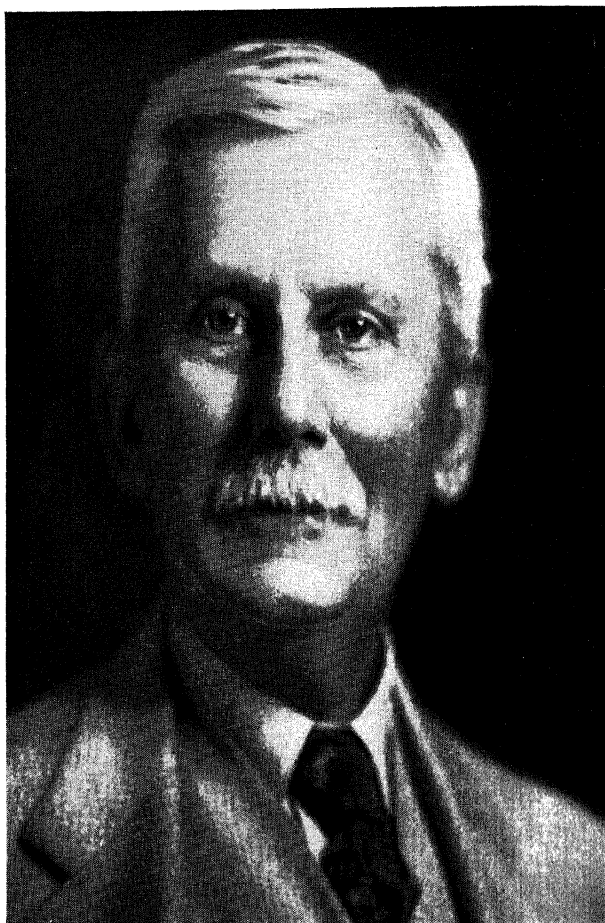
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Charles Shuckert

CHARLES SCHUCHERT

1858-1942

BY ADOLPH KNOFF

Charles Schuchert, paleontologist, stratigrapher, and foremost authority on paleogeography, died on November 20, 1942, in New Haven, Connecticut, in his eighty-fifth year. "A great man, a good man, a great scientist, a great investigator has gone," so voiced one of his most eminent students William H. Twenhofel, a sentiment that evoked the warmest responses in all who had known Professor Schuchert. Professor Schuchert attained his outstanding distinction as scholar and investigator without the benefits of high-school and college education. This achievement is extraordinarily surprising to many and naively mystifying to some. His life and what he made of it exemplifies in a remarkable way what can be achieved by native ability combined with determined effort.

He was born in Cincinnati, Ohio, on July 3, 1858, the oldest of six children. His parents were emigrants from Germany, who had come from the old country to Cincinnati only a few years previously. Philipp, the father, had arrived as a young man of 22 from Kranlucken, Saxe-Weimar, Saxony in 1855. Two years later he married Agatha Müller, then 20 years of age, who had come to this country from Reussendorf, in Franconia, Bavaria in 1854. Philipp was skilled as a cabinet-maker, and by hard work and enterprise he gradually built up a small but substantial business in making furniture. Young Charles, on reaching the age of six and a half years, was sent by his parents who were devout Catholics, to a parochial school. After finishing his schooling there at twelve, he began to attend a business school in order to prepare himself as bookkeeper in his father's firm. He had to help in the shop, however, opening it at seven in the morning and doing the necessary chores before going to school, and after classes he had to return to the shop to finish the day's work. Under this regimen he did not last out the year at the business school, but went to work in the varnishing

room of his father's furniture factory at a wage of two dollars a week. Thus ended all the formal education that Charles Schuchert was ever to receive. During the next six years he moved up from laborer to sales agent and finally to general manager.

In 1877 the factory was destroyed by fire and the blow so prostrated his father that young Schuchert, barely turned twenty, was left with the duty of supporting the family. During the next seven years he gradually rebuilt the business, but again a fire completely destroyed the factory. After this overwhelming disaster Schuchert was reduced to earning his livelihood by working in other furniture factories.

During these early years Schuchert had had a thrilling avocation. It had begun in 1866, when as a boy of eight, he stood watching a gang of laborers digging a ditch near his home. One of the men picked up something, and after looking at it intently, threw it to the boy with the remark, "Here Johnny, here's something for you." The boy ran home with his treasure, but all his father could tell him was that it was probably a *Versteinerung*, a petrification. A year later his father brought him another fossil. In the mind of the boy a proper collection must be labeled, so Charles labeled the fossils for his cabinet: one, "the petrification of a nanny-goat's horn" and the other, "the petrification of a nanny-goat's hoof." Subsequently, the "horn" when properly identified proved to be a coral and the "hoof" to be a bivalve shell.

Three years later Charles was taken by his father to see the geologic museum of William Foster. The great quantity of fossils there displayed opened to him an unknown world. These marvels, he was told to his astonishment, had been found in the hills about Cincinnati. Better still, Mr. Foster took father and son to the quarries at the head of Elm Street, where the rocks were full of bryozoans. "Then and there," said Schuchert long afterwards, "I learned the astonishing fact that these fossils were once animals, that they lived in a sea, and that this sea had once covered the greater part of Ohio—the most far-reaching sermon on the mount I ever listened to."

When about seventeen, he became acquainted with a "real paleontologist," one who published descriptions of fossils. This paleontologist, U. P. James, was a Cincinnati book dealer and publisher, who identified some of Charles' specimens for him. In that year also he acquired a copy of the *Paleontology of Ohio, Volume I*, printed in German for the benefit of the many Teutonic citizens of Ohio, and by means of it he began to learn how to identify his finds.

Up to the age of nineteen Charles' interest in fossils was that of a collector, but in 1877 he met Edward Oscar Ulrich, the man who was to turn him from an amateur into a professional paleontologist. Ulrich, who had had some rather desultory college training, had just reached his majority and had recently been put in charge of the collections of the Cincinnati Society of Natural History at a small salary. He was just beginning the career that made him one of the foremost paleontologists of his time. Schuchert had called on Ulrich to have some of his fossils named, and finding Ulrich to be highly agreeable and helpful, he called again and again and a warm friendship began. Ulrich went off to manage a silver mine in the Front Range of Colorado, but the mine ran out of ore and inside of two years he was back in Cincinnati in 1881. Schuchert and Ulrich again became inseparable companions. Ulrich began to publish papers on the local fossils, and Schuchert, who had learned to do lithographic work, urged Ulrich to use this process in illustrating his papers. Both men soon became proficient in this work. Schuchert then urged Ulrich to obtain the job of describing and illustrating the chapters on the Bryozoa in some of the proposed reports of several of the State Geological Surveys. As a result the Illinois Geological Survey in 1885 gave Ulrich the contract to illustrate Volume 8, and shortly afterward the Minnesota Survey commissioned him to describe and illustrate the Ordovician Bryozoa for the *Paleontology of Minnesota*. Ulrich was now swamped with work, and in May, 1885, Schuchert, who had had his factory and business wiped out by fire in 1884, became his assistant in lithography. In the next three years, so Schuchert records, the two of them "drew more than

one hundred plates of fossils for the Surveys of Illinois and Minnesota, and my divorce from the furniture business was complete."

In addition to assisting in the drawing, Schuchert sorted and prepared the material, but he was allowed no authorship. He made a pact with Ulrich whereby he acquired all Ulrich's brachiopods in exchange for all the non-brachiopods in his collection. Further, he began to exchange for brachiopods from abroad. In this way he built up an extraordinarily fine collection of brachiopods. These marine organisms have left a long and significant record in the rocks and are consequently of great importance in the determination of the ages of the older rock formations. The group was long the deepest interest of Schuchert.

In 1889 the great master of paleontology James Hall, State Paleontologist of New York, came to Cincinnati in search of material for a monograph on the phylogeny and evolution of the brachiopods. On seeing Schuchert's large and well-identified collection of brachiopods, he proposed that Schuchert come to Albany as his private assistant and allow him the use of the collection. Schuchert accepted and the arrangement lasted for nearly two and a half years. In other biographical accounts the more flattering statement is made that Hall wanted Schuchert because of his proved ability, but I have given the story as Schuchert has himself related it in his Memorial to John Mason Clarke and as he was wont to tell it in private conversation, along with other anecdotes of the incredible steps that James Hall would take to acquire collections of fine fossils.

The sojourn at Albany was highly profitable in the education of Charles Schuchert. Not that James Hall aided it greatly, but Schuchert made the friendship of John M. Clarke, brilliant paleontologist then beginning his long and distinguished career on the Geological Survey of New York. To Clarke, Schuchert's stay in Albany for thirty months "was one of intimate and inspiring association and his departure in 1891 took away the only sympathetic soul there was in the place." The result of the joint labors of the trio was a memoir of 760 pages and

90 plates, entitled "An Introduction to the Study of the Genera of Paleozoic Brachiopoda" by Hall and Clarke.

At Albany he met also Charles Emerson Beecher, who was to become one of his most intimate friends. Beecher was on the staff of Professor O. C. Marsh at Peabody Museum of Yale University, but had formerly been a member of the New York State Survey, and visited Albany from time to time on official business.

When the work at Albany was finished, Schuchert was invited to Minneapolis by N. H. Winchell, State Geologist of Minnesota, to study the brachiopods of Minnesota. His salary, which had been a bare \$15 a week during the past six years, was doubled, and better still, he was given the privilege of authorship and was allowed to publish the results of his investigations.

After finishing his work in Minnesota, Schuchert in 1893 joined the staff of the U. S. Geological Survey in Washington as Assistant Paleontologist, becoming assistant to Charles D. Walcott, who was then Chief Paleontologist of the Survey. In April of that year he was sent to the Peabody Museum of Yale University to spend a year in assisting Professor Beecher to prepare the remarkable Early Carboniferous crinoids from Crawfordsville, Indiana in the Marsh Collections, so that they could be used for display in the Government exhibition at the Chicago Exposition. By arrangement the specimens were to be divided between the Peabody Museum and the U. S. National Museum. While thus engaged in New Haven Schuchert had the good fortune to occupy for six months the room of L. V. Pirsson, who was away in Europe. Beecher the paleontologist, Penfield the mineralogist, Pirsson the petrologist, and Wells the chemist had fitted up living quarters for themselves in the attic of Old Sheffield Hall. After the return of Pirsson from his studies in Heidelberg and Paris, Schuchert continued for several months longer to foregather with the Attic philosophers as they called themselves, and to enjoy in this brilliant company pleasant companionship and intellectual stimulation. Beecher by this time in his career had arrived at the point in which he was no longer interested in describing fossils—in fact, he often

told Schuchert that he wished that all fossils had been named—and was engaged in ontogenetic and phylogenetic studies based on the law of recapitulation. These broad views, somewhat strange to find in a disciple of James Hall, stemmed from the philosophic paleontologist Alpheus Hyatt and doubtless greatly enlarged Schuchert's outlook upon paleontology.

The job of preparing the crinoids was finished in May, 1893. Schuchert then returned to Washington and spent the field season of that year in visiting all the important localities of Middle and Upper Cambrian rocks in Wisconsin, Minnesota, and Iowa to make complete collections of the fossils characteristic of the St. Croix terrane [now called "Croixan," of late Cambrian age]. In 1894 Schuchert was transferred from the Geological Survey to the staff of the U. S. National Museum, as Walcott had been appointed Director of the Survey. He became Assistant Curator in charge of Invertebrate Paleontology. During the ensuing eleven years he was in charge of the great collections of fossils that had been assembled through the field work of the various government surveys. Here at the Museum he enjoyed two great advantages: on the one hand abundant paleontologic material collected from a vast area and, on the other, unrivaled library facilities. An even greater advantage was the daily association with eleven paleontologists, all actively engaged in research.

In 1904 Schuchert was called to Yale University to fill the positions left vacant by the all-too-early death of Professor Beecher. In those days Yale University had two departments of geology—one in Yale College and the other in the Sheffield Scientific School. Peabody Museum, housing the important geologic collections, was another essentially autonomous unit. Schuchert was appointed Professor of Paleontology in the University and Curator of the Geological Collections in the Peabody Museum, also Professor of Historical Geology in the Sheffield Scientific School and a member of the Governing Board of that School. The salary was \$2750, "to be increased to \$3750 when Mr. Schuchert gives the instruction in historical geology to Sheffield seniors as done by the late Professor Beecher."

Schuchert was then forty-six. To fulfill the teaching requirement, he had to make, as he himself characterized it, "the somewhat painful metamorphosis from curator to professor." The difficulty of presenting to undergraduates a broad over-all picture of the ever-changing distribution of lands and seas during geologic time stimulated the new professor to prepare a set of paleogeographic maps to give visual presentation of the past geographies. The improvement of these maps, as data kept accumulating from year to year, became his most engrossing task throughout the remainder of his life. In 1909 Schuchert was made chairman of the Geology Department of the Sheffield Scientific School, and when the University was reorganized in 1918-1919 and the departments were unified, Schuchert became the chairman of the University department, serving until 1921, when he relinquished it at his own request. During 1914-1915 he served as Acting Dean of the Graduate School.

On reaching the age of sixty-five in 1923, Professor Schuchert decided that because of the large amount of research work he had in hand he would ask to be released from his teaching duties. Then he was appointed Professor Emeritus of Paleontology, and in place of Curator of the Geological Collections he became Curator of Invertebrate Paleontology. In 1926 he retired as Curator Emeritus. Thus freed of all teaching and administrative duties, he began the third period of his life. Rich in experience, equipped with an encyclopedic store of information on his chosen fields; keenly interested, enthusiastic, and open to all new ideas; and zealous in his work, Professor Schuchert in the nearly two decades that remained to him produced a great body of work which few younger men can hope to rival in the same span of time.

During the first few years of his retirement he would give a series of lectures in the winter months at universities in the Southwest,—at the University of Texas, University of Arizona, and others. Later, however, he gave up this program, and would leave New Haven early in December, for Florida, and return to New Haven about the middle of April. These winter sojourns in the South were far from the nature of vacations

however, as he always took along with him ample material to keep him fully employed. They undoubtedly helped to maintain the good health that he continued to enjoy until the final months of his life.

Schuchert was never married. To G. Arthur Cooper he explained that he was "married to the science," and this explanation sums up his life-long absorption in his work and his wholehearted devotion to his chosen mate. He was no misogynist, however; he was in fact friendly to all, warmhearted, and generous. He was in particular devoted to the graduate students in paleontology and stratigraphy, among whom were included not only his own students but those who came during the years of his retirement. He encouraged and heartened them by his kindly interest in their work, their problems, and their aspirations. Many of them he aided financially in their field work. Characteristically the aid thus given remained unknown to outsiders, except as the recipients themselves caused it to become known.

In his will Professor Schuchert left to Yale University the bulk of his estate, amounting to more than \$50,000, to establish the Charles Schuchert Fund, to be used first, to endow a fellowship in Invertebrate Paleontology and Stratigraphy and second, to support research work on or for the increase of the collections in invertebrate paleontology.

SCIENTIFIC WORK

Professor Schuchert's first interest in science, as the early history of his life has shown, was in paleontology and was centered in the brachiopods. The fine collection of brachiopods that as a young man he had assembled in Cincinnati moved the great paleontologist James Hall to acquire it and to take it to Albany by appointing its owner on the staff of the New York State Museum in Albany. Later when Schuchert had become Assistant Curator of Stratigraphic Paleontology in the United States National Museum, he was enabled to bring out in 1897 his first major contribution, "A Synopsis of American Fossil

Brachiopoda, Including Bibliography and Synonymy," a volume of 464 pages. This publication, as Twenhofel pointed out, "brought to some degree of fruition the work begun in his boyhood—his determination to master all that was known of the Brachiopoda—and placed him among the foremost students of this group of invertebrates." Much later, in 1932, he published with the assistance of G. Arthur Cooper, a memoir of 270 pages on the "Brachiopod Genera of the Suborders Orthoidea and Pentameroidea," which is regarded as his greatest contribution to paleontology.

His devotion to the Brachiopoda never waned, but as time went on his interests widened more and more to the implications of the paleontologic record. Stratigraphic problems, the delimitation of the geologic systems, the climates of the geologic past, the geosynclinal doctrine, and the dating of mountain-making movements were some of these new interests.

Transcending all these, however, were his paleogeographic studies. From them resulted a series of paleogeographic maps, on which he showed the distribution of land and sea during the geologic past. As already mentioned, the stimulus to prepare maps of this kind came to Schuchert soon after he began to teach historical geology at Yale in 1904, when he attempted in his lectures to acquaint his students with the distribution of the geologic formations of North America. In order to visualize to the students where the formations occur and their geographic extent he drew a series of paleogeographic maps. The results proved at once that this mode of presentation is strikingly helpful. In 1910 he published his now classic "Paleogeography of North America," which is illustrated by 50 paleogeographic maps. These maps show the lands and seas at 50 successive stages in the history of North America, ranging in time from early Cambrian to Pliocene. Although many paleogeographic maps had been drawn since the first of this kind had been made by James D. Dana in 1863, the maps here presented marked a great step in advance, for they were based on much closer time limits and more accurate correlations than had been employed in making any previous map. The encyclopedic command of in-

formation and the synthesis of the vast array of material are typical of Schuchert's work. These maps confessedly were regarded by him as imperfect and were published to elicit comment and further information from which to construct improved versions. It became Schuchert's absorbing task during the remainder of his life to add to these maps day by day all new information as it came along. By 1913 their number had already increased from 50 to 85, and eventually it grew to 130. These maps surpassed in detail and accuracy all earlier maps and they brought their author international acclaim.

By 1928 Schuchert could say that "no paleogeographic map is worth the paper on which it is printed unless it depicts the actual state of affairs for a limited geologic time, say several hundred thousand years." Taken literally, this dictum would necessitate the preparation of more than 1000 maps to portray the successive geographies from the beginning of the Cambrian to the present. The maps show that the North American continent has been many times extensively flooded by the sea; and that after the sea had advanced a maximum distance inland, it withdrew. Each transgression of the sea and the succeeding regression constitutes a rhythm, though the terms "periodic" and "rhythmic" thus applied are somewhat loosely used. The idea of rhythms in geologic history strongly colored Schuchert's thinking and through his writings many others, but the "periodicity" he envisaged, as just pointed out, is far from the periodicity of physics.

Construction of the paleogeographic maps led Schuchert inevitably to the study of geosynclines and their significance in geologic history. The fundamental tenet of the geosynclinal doctrine, that belts of thick sedimentary accumulation determine the sites of future mountain ranges built of folded strata, was outlined by James Hall in 1857 and was expanded by Dana, who in 1873 named the troughs in which the load of strata were deposited "geosynclines." In his presidential address before the Geological Society of America in 1922 entitled "Sites and Nature of the North American Geosynclines," Schuchert illumined this doctrine in the light of his great stratigraphic

knowledge. A vast amount of stratigraphic information had accumulated since the time of Hall and Dana and this was utilized to locate the sites of the North American geosynclines that came into existence from Proterozoic to Late Cretaceous time. Among the important conclusions drawn are: one, that the amount of sediment laid down in the geosynclines appears to have nothing to do with when they were folded, the amount ranging in thickness from less than 10,000 feet to 76,000 feet in the Cordilleran geosyncline [later work has greatly reduced this figure, as the immense amount of erosion prior to the deposition of the middle Cambrian Flathead quartzite had not been recognized at the time Schuchert wrote]; and two, the length of time has nothing to do with the vanishing of the geosyncline, i.e., the life of a geosyncline may range from short (a portion of a geologic period) to very long, comprising six or more geologic periods.

The history of geosynclines led to studies of the deformation of the strata laid down in them, of the "orogenies" as they are called. An outcome of this interest was his paper on "Orogenic Times of the Northern Appalachians," in which he showed that the belt from New Jersey to Newfoundland had been affected by three orogenies—Taconian, Acadian, and Appalachian. Of the three, the Acadian orogeny was recognized to have been the most powerful, though according to the doctrine he had himself laid down in 1915 in his widely influential text book on Historical Geology the Acadian orogeny, occurring *within* an era, should have been a mere "disturbance" and the Appalachian orogeny, occurring (as he then thought) at the *end* of an era should have been a far stronger orogeny, a "revolution." This paper marked a growth in Schuchert's geologic philosophy and illustrates his life-long tendency, undiminished to the end, to advance with advancing knowledge.

A work that engrossed Schuchert during the closing years of his life was the writing of his "Historical Geology of North America." In reality thirty years of his life had already gone in its preparation. Two volumes were published and the third was in preparation when death called him. As the plan was

originally conceived, the volumes were to be built around the paleogeographic maps; they were to present the factual data on which the maps were based. The first volume, "Historical Geology of the Antillean-Caribbean Region," comprising 811 pages, was published in 1935; it synthesizes the work of countless geologists and gives us a picture of the geologic development of the region as a whole. The volume is therefore valuable not only as a synopsis and a reference book but also for its broad generalizations and its stimulus to other workers. The second volume, "Stratigraphy of the Eastern and Central United States," of 1013 pages, appeared in 1943. These volumes, wrote Professor Schuchert, "are the end-results of my life-long work in Geology." They were to him labors of love, but to those to whom they are indispensable they should be gratefully regarded as gifts of scientific altruism. We shall not soon look upon their like again.

HONORS

Many honors came to Charles Schuchert. He was elected president of the Paleontological Society in 1910 and president of the Geological Society of America in 1922, and served as vice-president of the American Association for the Advancement of Sciences in 1927. He was elected to the National Academy of Sciences in 1911. His eminence was also recognized abroad by election to membership in many scientific societies, in Austria, Belgium, China, Germany, Great Britain, Norway, Russia, and Sweden.

The degree of LL.D. was conferred on him by New York University in 1914, the Sc.D. by Yale in 1930 and by Harvard in 1935. The Hayden Gold Medal was awarded to him by the Philadelphia Academy of Sciences in 1929; the Mary Clark Thompson Medal was given to him by the National Academy of Sciences in 1934; and in the same year the Penrose Medal of the Geological Society of America was conferred on him, "in recognition of eminent research in pure geology" and "of outstanding original contributions or achievements which mark a decided advance in the science of geology."

EPILOGUE

"Professor Schuchert was unquestionably 'one of the great' of his time. No one could have dreamed in 1884, when Charles Schuchert watched his business reduced to smoke and ashes, that here was a man who almost sixty years later would be the foremost paleogeographer of his day, a great paleontologist and stratigrapher, a most inspiring teacher, and a man revered by many former students."—*William H. Twenhofel*.

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KEY TO ABBREVIATIONS

- Am. Acad. Arts Sci. Proc. = American Academy of Arts and Sciences, Proceedings
 Am. Assoc. Petrol. Geol. Bull. = American Association of Petroleum Geologists, Bulletin
 Am. Geol. = American Geologist
 Am. Inst. Min. Met. Eng. Trans. = American Institute of Mining and Metallurgical Engineers, Transactions
 Am. Jour. Sci. = American Journal of Science
 Am. Phil. Soc. Proc. = American Philosophical Society, Proceedings
 Am. Phil. Soc. Yearbook = American Philosophical Society, Yearbook
 Ann. Mag. Nat. Hist. = Annals and Magazine of Natural History
 Arch. Zool. Exper. Gen. = Archives de Zoologie Expérimentale et Général
 Biol. Soc. Wash. Proc. = Biological Society of Washington, Proceedings
 Econ. Geol. = Economic Geology
 Geogr. Rev. = Geographical Review
 Geol. Fören. Förh. = Geologiska Föreningens in Stockholm Förhandlingar
 Geol. Soc. Am. Bull. = Geological Society of America, Bulletin
 Geol. Soc. Am. Proc. = Geological Society of America, Proceedings
 Geol. Soc. London Quart. Jour. = Geological Society of London, Quarterly Journal
 Geol. Sur. Canada Bull. = Geological Survey of Canada, Bulletin
 Jour. Geol. = Journal of Geology
 Jour. Paleon. = Journal of Paleontology
 Jour. Sed. Pet. = Journal of Sedimentary Petrology
 Minn. Geol. Nat. Hist. Sur. = Minnesota Geological and Natural History Survey
 Nat. Acad. Sci. Biog. Mem. = National Academy of Sciences, Biographical Memoirs
 Nat. Acad. Sci. Proc. = National Academy of Sciences, Proceedings
 Nat. Res. Coun. Bull. = National Research Council, Bulletin
 N. Y. State Mus. Bull. = New York State Museum, Bulletin
 Pan-Am. Geol. = Pan-American Geologist
 Peabody Mus. Nat. Hist. Bull. = Peabody Museum of Natural History, Bulletin
 Pop. Sci. Mo. = Popular Science Monthly
 Sci. Am. Suppl. = Scientific American Supplement
 Sci. Mo. = Scientific Monthly
 Smithsonian Inst. Ann. Rept. = Smithsonian Institution Annual Report
 Smithsonian Misc. Coll. = Smithsonian Miscellaneous Collections
 U.S. Geol. Sur. Ann. Rept. = United States Geological Survey Annual Report

U.S. Geol. Sur. Prof. Paper = United States Geological Survey Professional Paper

U.S. Nat. Mus. Bull. = United States National Museum, Bulletin

U.S. Nat. Mus. Proc. = United States National Museum, Proceedings

Wash. Acad. Sci. Jour. = Washington Academy of Sciences, Journal

Yale Rev. = Yale Review

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